THREE ESSAYS IN ECONOMICS

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I present findings from three separate empirical studies, each of which apply economic theory to real world applications to address the following questions: (1) To what extent is there a shortage of marriageable men in the U.S.? (2) Can we apply behavioral economic principles of choice architecture that have proven effective in physical environments to online settings? (3) What is the effect of medical marijuana laws on body weight and physical activity?

BIOGRAPHICAL SKETCH

I am an empirical economist who seeks to understand the economic principles that influence human decision-making in a variety of contexts. I'm currently teaching microeconomics at Southern Utah University.

I specialize in applying microeconomics and behavioral economics (a mix of economics and psychology) to problems we face every day as individuals and a society. In research to date, I've explored questions related to health economics, behavioral economics (with a focus on designing **choice architecture** to improve the quality of decision making in nutrition settings), the economics of education, and labor economics. When asked about my research interests, I usually save time by saying I study applied microeconomics. I am also fascinated by neuroeconomics and psychology, particularly a branch of the literature that models the brain as a Bayesian updater.

My current research aims to add to what is known about the interplay between expectations and preferences in determining individuals' choices and behaviors. Inspired by Schopenhauer's observation that "the world is as we perceive it," my current research agenda focuses on obtaining better measures for individuals' perceptions. To this end, I run **online prediction tournaments** using a gamified survey approach I have been working on to collect rich expectations data. These data offer a unique perspective on an individual's perceptions of the decision-relevant tradeoffs they face daily when making choices.

My wife, Micquel, and I have 3 kids: Hal (7 years), Lulu (5 years), and Maeby (2 years). We enjoy time together building things with Lego bricks and learning to juggle a soccer ball. We recently finished reading the *Harry Potter* series aloud together. Not sure what our next book will be.

To Micquel and our kids—for something to read while on the toilet, and, barring that, if you run out of toilet paper, that the paper this is printed on may help you maximize your utility in a world where binding constraints can and do happen—please know that I love each of you so much

ACKNOWLEDGMENTS

I'd like to thank my graduate school mentors, Mike Lovenheim, Francesca Molinari, and David Just for their unwavering support through my graduate school years. I'd also like to tip my hat to my co-authors, but especially to the two brother-economists, Joe Price and Josh Price who have consistently seen good in me and helped manifest it. There are many others along my path (too many to mention here) who have taught me so much and given me incredible encouragement as I faced challenges with research and life, but chief among these are my family and I feel a debt of gratitude to them that makes my substantive graduate school debt seem paltry in comparison. The people in my life are what has given it meaning, and it is thanks to them that I have come this far.

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PREFACE

This work may seem to the uninformed reader like three disjoint papers tenuously strung together to form a dissertation that could pass muster and allow a student to complete the requirements of their degree. If you are feeling like this uninformed reader, let me inform you: it also appears that way to the informed readers as well, because that is precisely what this is. But it is also much more. It is the produce of the blood, sweat, and tears of a man trying to support his family by obtaining the education he needs to contribute to a field that he cares about. I am not delusional enough to believe that this dissertation will be read by more than my eyes right now and, perhaps someday, some errant grad student who has gone so tangentially far afield that even she is realizing that she really should try to start working on something more directly related to her own dissertation. But because your eyes are reading this, I'd like to go ahead and take this opportunity to convey to you a couple things that I did learn, beyond the findings presented in the papers that follow:

- Life happens to you, but sometimes you can happen to life a bit, too: both of my daughters were diagnosed with Cystic Fibrosis while I was in grad school. This was a huge blow to us, but it has also endowed us with a perspective that you couldn't pay me to give up. I appreciate each moment more poignantly now that the prospect of a shortened life-span together looms over us. As crazy as it sounds, the poet was right "sometimes the things our life misses help more than the things which it gets."
- You can't directly pursue anything: not sure if this is totally true, but I think a close corollary to it may be. The corollary is essentially this: you need to learn how to fail fast, fail frequently, and fail well. I made it through school by failing forward and persistently working on what I could control. Things turned out different than I intended in terms of the research that is included in these chapters, but I tried to be flexible enough to seize opportunities that presented themselves and it turns out that it was enough in the end.
- Keep going, friend. Never give up. I'm cheering for you. But do get back to working on *your* dissertation now.

Lastly, any mistakes herein are my own. Even if they weren't, adopting this stance of taking ownership has been important for me in getting to this point.

Mismatches in the Marriage Market

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MISMATCHES IN THE MARRIAGE MARKET

Objective: This paper provides an assessment of whether unmarried women currently face demographic shortages of marital partners in the U.S. marriage market.

Background: One explanation for declines in marriage is the putative shortage of economically-attractive partners for unmarried women to marry. Previous studies provide mixed results, but are usually focused narrowly on sex ratio imbalances rather than identifying shortages on the multiple socioeconomic characteristics that typically sort women and men into marriages.

Methods: This study identifies recent marriages from the 2008-2012 and 2013-2017 cumulative 5-year files of the *American Community Survey*. Data imputation methods provide estimates of the sociodemographic characteristics of unmarried women's potential (or synthetic) spouses who resemble the husbands of otherwise comparable married women. These estimates are compared with the actual distribution of unmarried men at the national, state, and local-area levels to identify marriage market imbalances.

Results: These synthetic husbands have an average income that is about 58% higher than the actual unmarried men that are currently available to unmarried women. They also are 30% more likely to be employed (90% vs 70%), and 19% more likely to have a college degree (30% vs 25%). Racial and ethnic minorities, especially black women, face serious shortages of potential marital partners, as do both low- and high-SES unmarried women, both at the national and sub-national levels.

Conclusions: This study reveals large deficits the supply of potential male spouses. One implication is that unmarried may remain unmarried or marry less well-suited partners.

MISMATCHES IN THE MARRIAGE MARKET

Recent declines in U.S. marriage are reflected both in delayed marriage and increases in permanent singlehood, punctuated by intermittent spells of nonmarital cohabitation (Lichter & Qian, 2008; Manning, Brown, & Payne, 2014). One argument is that the traditional economic foundations of marriage have been eroded by a deteriorating job market, a consequence of automation, deskilling, deunionization, and global competition for cheap labor (Lundberg, Pollak, & Stearns, 2016; Sweeney, 2002). Indeed, Wilson's (1987) "marriageable male" hypothesis provides a useful theoretical and empirical benchmark, claiming that declines in marriage are driven, at least in part, by reductions in employment prospects and earnings among men, especially less skilled racial and ethnic minorities at the bottom of the education distribution. High rates of incarceration and substantial out-marriage to white women, especially among black men, have also left many minority women without marital partners (Crowder & Tolnay, 2000). The fact that women's educational levels now exceed men's (Buchman & DiPrete, 2006; Van Bavel, Schwartz, & Esteve, 2018) further implies that young women—by necessity—are less financially dependent on husbands than in the past and that educational hypogamy has become more commonplace (Breen & Salazar, 2011; Qian, 2017). Young women seemingly face shortages of demographically-similar men to marry.

This paper provides new estimates of spousal mismatches in the marriage market. Specifically, we compare the demand-side sociodemographic characteristics that women typically seek in male partners with the availability or supply of these characteristics in the marriage market. We use methods for imputing missing data (in effect, creating "synthetic husbands") to infer the likely sociodemographic profiles of the husbands of unmarried women if they married. We make the assumption that these women would marry men comparable to the husbands of demographically-similar women who are currently married. We accomplish our goals using national and sub-national data from the most recently released cumulative 5-year files (2008-2012 and 2013-2017) of the annual *American Community Survey*. By identifying the counterfactual case (i.e., the likely demographic profile of husbands if unmarried women became married), we provide a direct assessment of whether women currently face demographic constraints in the marriage market. Our study—for the first time—identifies both surpluses and deficits of so-called synthetic husbands in the marriage market.

This didactic exercise shows that unmarried women face overall shortages of economically attractive partners with either a bachelor's degree or incomes over $40,000 a year. Most previous work suggests that women are more likely to remain unmarried than to "settle" by marrying partners who are mismatched on age, education, or race (Lewis & Oppenheimer, 2000; Lichter, Anderson, & Hayward, 1995). A recent study by Qian (2017), however, now indicates that patterns of assortative mating have shifted, switching from a tendency in 1980 for women to "marry up" in socioeconomic status to a pattern today of "marrying down". This reversal suggests, at a minimum, that growth in the pool of marriageable men has not kept pace with the rapid rise in women's socioeconomic status. Our study reinforces the commonplace view that women today face new marriage trade-offs at a time when finding a suitable marital match has become more difficult.

BACKGROUND

Marriage Market Imbalances and Marital Search

Large but declining majorities of both single and cohabiting young women (and men) intend, expect, or plan to marry (Kuo & Raley, 2016; Manning, Longmore, Giordano, 2007; Vespa, 2014). This implies that recent marriage trends and mate selection processes may simply result from shifting marital attitudes and preferences. They may also reflect third-party constraints, such as parental and religious influences, changing cultural norms, and legal restrictions on marriage (Kalmijn 1998) and, as we assume here, uneven marriage market opportunities and constraints (Lichter & Qian, 2019). Indeed, the desire to marry is not always realized, which explains why marriage rates often fall well short of women's marital expectations or plans to marry (Gibson-Davis, Edin & McLanahan, 2005). This is the case among poor single mothers, who typically hold conventional aspirations for marriage but are much less likely than middle-class single women to actually marry (Lichter, Batson, & Brown, 2004). Deficits in the supply of economically-attractive men may be the reason why.

From a search-theoretic demographic perspective, the marriage market is similar to the matching of employers and employees in the labor market (England & Farkas, 1986; Lichter et al., 1992). In a two-sided matching process, both employers and employees arguably seek the "best" match possible. Workers with unequal skills, different wage demands, and other qualifications are sorted into jobs that presumably match the particular needs of employers (i.e., that the supply and demand for workers are in equilibrium) in competitive labor markets. Similarly, marriage-seeking men and women usually sort on similar characteristics in the marriage market. They presumably seek marriage partners who match their socioeconomic status, age, race, and attractiveness, among other valued traits (Schwartz, 2013). A fair or equitable exchange is revealed in positive assortative mating or marital homogamy.

Of course, there is no assurance that marriage markets are in demographic equilibrium—where the demand for partners with particular sociodemographic profiles matches the supply. National and local area demographic shortages of potential marital partners, for example, mean that some women will necessarily remain unmarried or will have to search longer for a suitable partner. Shortages of marriageable men imply increasing singlehood and delayed marriage (as indicated by the rise in age at first marriage). Alternatively, women may instead "settle" for a marital match that falls short of their aspirations in a spouse (i.e., the "reservation quality partner," to use the terminology of England & Farkas [1986]). This will be expressed in new patterns of marital hypogamy or downward marital mobility.

Measuring Disequilibria in the Marriage Market

How best to measure marriage market mismatches is not obvious, although it will undoubtedly require taking into account surpluses (or deficits) in the demographic supply of both men and women with specific traits that are commonly exchanged in marriage. In the contemporary U.S. marriage market literature, for example, job stability, earnings, and education play a large and singular role in the mate selection process (Charles, Hurst, & Killewald, 2013; McClendon, Kuo, & Raley, 2014). Nearly 80 percent of unmarried women indicate that a "steady job" would be very important to them in choosing a spouse (Wang & Parker, 2014). A partner with a good job is usually viewed as a necessary but insufficient condition for marriage (Schneider, Harknett, & Stimpson, 2019). Qualitative research also suggests that women often gauge the "marriageability" of potential male partners by the effort put into finding and keeping a job, as well as by the source of income, i.e., earnings from a stable job or from illicit or illegal activities (Smock, Manning, & Porter, 2005; Thomas, 2012).

For some low-income women, marriage may be a problem (i.e., exposure to abuse) rather than a solution (e.g., reducing poverty and inequality). Low or declining earnings among potential male partners also may heighten fears of divorce while discouraging women from getting married (Waller & Peters, 2008). For cohabiting couples, a good job is typically a requirement before committing to marriage or for making marriage financially feasible (Smock et al., 2005). The implication is clear: mismatches in the marriage market in the form of shortages of economically attractive men may exacerbate uncertainty and heighten disincentives to marriage, especially at a time of rising education and growing financial independence among American women (Gibson, Edin, & McLanahan, 2005; Schwartz, Zeng, & Xie, 2016; Watson & McLanahan, 2011).

More generally, we recognize that U.S. marriage market conditions—the demographic composition of potential male partners—have undergone substantial change over the past three decades. Conventional social norms surrounding marriage, including positive assortative mate selection based on the shared sociodemographic traits of partners (e.g., age, race, education, and income), have arguably been upended or altered by new economic realities and growing family complexity (Qian & Lichter, 2018). Marriage market mismatches—demographic shortages or surpluses of potential spouses—are likely to be distributed unevenly in the unmarried population. For example, economic globalization has disproportionately affected working class men and laborers at the bottom of the education distribution (Autor, Dorn, & Hanson, 2017; Oppenheimer et al., 1997). Under these circumstances, it is hardly surprising that the conventional model of "husband as breadwinner" and "wife as homemaker" has increasingly given way to more equalitarian marriages or to other less traditional family arrangements, such as cohabitation and single parenthood (Goldscheider, Bernhardt, & Lappegård, 2015). This is likely to be the case in particular among professional and highly-educated women. Marriage market mismatches are likely to be expressed unevenly, which ultimately contributes to diverse patterns of partnering and parenting among American women (Sassler, 2010; Smock & Greenland, 2010).

Race also continues to play a non-trivial role in America's highly segmented marriage market. Racial and ethnic disparities in the quantum and tempo of marriage have accelerated over time (Raley, Sweeney, & Wondra, 2015). America's historically disadvantaged racial and ethnic minority populations remain highly stratified by the usual economic predictors of marriage: education, job stability, earnings, and poverty. At the same time, interracial marriages have increased significantly since Loving v. Virginia (in 1967), which abolished anti-miscegenation laws. The extraordinary recent growth of Hispanic and Asian immigrant populations also has added diversity to the pool of potential marriage partners (Charles & Luoh, 2010; Qian, Lichter, & Tumin, 2018). The racial dimension of marriage and mate selection processes has likely contributed to further imbalances in the marriage market (i.e., as shortages in one segment of the market create new demands for a mate in other segments).

Charles & Luoh (2010) also have shown that mass incarceration of black men has depleted the pool of unmarried men in inner-city urban neighborhoods, which has greatly reduced the prospect of marriage among black women. On average, black men are roughly seven times more likely than white men to be incarcerated (Raley et al., 2015; Lopoo & Western, 2005). Race remains a significant demographic dimension of national and local marriage market mismatches, especially as educational and income constraints are amplified within many low-income and residentially-segregated minority populations (Wilson, 1987). Indeed, numerical shortages of same-race potential partners with attractive socioeconomic and demographic profiles represent an especially salient dimension of the mismatches among disadvantaged minority women.

Current Study

Our overall goal is largely descriptive: To appropriately characterize U.S. marriage market conditions for currently unmarried women with different sociodemographic profiles. We have two specific objectives.

First, we use data imputation methods to infer what the sociodemographic characteristics of each woman's spouse would be if they married a man with similar characteristics to the husbands of comparable women. We build on an imputation method used by Sassler and McNally (2003) to reclaim missing partner information for cohabiters and on other approaches that create so-called "synthetic spouses" when only one spouse in the household is available for analysis (Hamermesh & Pfann, 2005). Rather than focusing narrowly on sex ratio imbalances (Cohen & Pepin, 2018), we identify shortages on the many possible characteristics (e.g., age, education, income, etc.)—both at the national and sub-national levels—that typically sort women and men into marriages.

Second, we compare the distribution of characteristics of synthetic husbands with the distribution of all unmarried men in our sample. The goal is to identify the shares of women without a suitable marriage match, and the specific female subpopulations that face the greatest risk of a "tight" marriage market—one with a demographic shortage of men to marry. Our discussion of marriage market imbalances focuses primarily on (1) low-educated or poor women who are sometimes the target of recent marriage promotion programs (Lichter Graefe, & Brown, 2003; Ooms, 2019) and (2) highly-educated women who have ostensibly "priced" themselves out of the marriage market and now face shortages of economically attractive men to marry (DiPrete & Buchmann, 2006; Musick, Brand, & Davis, 2012). Or, stated differently, men may have become less competitive in the marriage market, falling behind on those economic and demographic traits that made them attractive to women as marriage partners.

METHODS

Data and Sample Restrictions

Our analyses use the *American Community Survey* (ACS) 5-year samples covering the years 2008-to-2012 and 2013-to-2017. The ACS provides a rich set of sociodemographic characteristics for all unmarried and currently-married women and their spouses, including sex, age, race/ethnicity, education, income, employment status, and number and age of children. Sample sizes are sufficiently large to observe the alignment of the national and sub-national (state and local) supply and demand of opposite-sex partners in marriage market.

We split our sample into four groups based on sex (males and females) and marital status (i.e., married, spouse-present; unmarried). We do not consider same-sex couples, which are not identified for all years and sub-national areas during our 10-year study period. Our married-couple sample also does not include cohabiting couples, which are included here with other unmarried persons. Previous studies indicate that cohabiting couples are often highly unstable and less likely than in the past to lead to marriage (Guzzo, 2018; Lichter, et al., 2016). Interracial and other forms of heterogamy also are more likely to cohabit rather than marry (Blackwell and Lichter 2000). Moreover, marriage is linked to higher rates of commitment and fertility, and confers certain legal rights and obligations that are not imposed on cohabiting couples. In some additional analyses (not shown, but available upon request), we treated cohabiting couples as "married" and found results that are similar to those reported here.

An important feature of our study is that we restrict the sample of currently-married women to those who married in the last five years. Unlike studies of intact marriages (Qian & Lichter, 2007), the characteristics we observe for marriage markets and for actual and synthetic spouses more closely match characteristics at the time of marriage. Our sample of unmarried individuals includes those between the ages of 25 and 45, while the sample of married individuals are drawn from recently-married couples in which at least one spouse is aged 25-45. By age 25 the majority of women are still unmarried but will have achieved their highest level of education. By age 45, however, over 95% of ever-married women will have married (Goldstein & Kenney, 2001). These age restrictions also have the benefit of reducing biases from age-selective patterns of divorce and mortality.

Matching Spousal Characteristics

Table 1 provides summary statistics for each of the key variables used in our matching exercise, reported separately for each of the four groups. Married men and women are on average older than their unmarried counterparts, and they have higher education levels. Unmarried women are slightly more likely to be employed but earn slightly less than their married counterparts. These observed similarities and differences are largely consistent with conventional wisdom that married men are more "economically attractive" or "marriageable" than unmarried men, and that most single women (by definition) must rely on their own employment and earnings to support themselves and their families.

(Table 1 about here)

For example, the average total personal income of married men is $70,000 compared to $35,000 for unmarried men (measured in 2017 dollars). Nearly 40 percent (37%) of married men are college graduates compared to only 25% of unmarried men. Although the difference is small in absolute terms, the relative difference in employment status is large. About twice as many unmarried as married women are unemployed (7.05% vs. 3.79%). The largest relative difference between married vs. unmarried women is the percentage black (6% vs. 18%), a result that highlights the persistent marriage gap between blacks and whites.

Imputing Synthetic Spouses

The key empirical goal is to determine the characteristics of the spouse to whom the unmarried women in our sample would likely be married, assuming they exhibit the same mate selection patterns as currently married women. Current patterns of marital homogamy represent the statistical if not cultural norm. We identify these counterfactual husbands (i.e., synthetic spouses) by matching each unmarried woman to the married woman or set of married women who have a similar set of observable characteristics. These characteristics are based on several conventional matching variables, including age, race, education, income, and employment status (Lichter & Qian, 2019). We also include military veteran status, acknowledging that military veterans are likely to consider veteran status when selecting a spouse (especially if we assume that veterans exhibit certain traits, such as a strong sense of pride, honor and integrity, which enhance their attractiveness in the marriage market) (Moore, 2011). Military service also provides opportunities for marrying other veterans (e.g., interactions on military bases or, later, at veterans' organizations such as the VFW).

The ACS data include social and demographic characteristics that provide the basis for marital matches, but lack other traits that may be involved in the marital decision-making process. For example, the ACS lacks indicators of personality traits, intelligence, or physical attractiveness (e.g., weight, beauty, or physical features). Of course, these unobserved traits may be correlated with getting and keeping a good job or earning a wage premium (e.g., in the case of height among men). Goldscheider & Waite (1986) argued that employment provides the resources to start and maintain a stable household and a clear indicator of economic prospects in the future. Steady employment may be indicative of other desirable factors, such as ability, motivation, and reliability, which also make for more attractive or sought-after marital partners.

We estimate the characteristics of synthetic spouses using two alternative approaches. Our first approach is to use a standard hot deck imputation in which we randomly draw a spouse out of the set of possible matches and repeat this process for all unmarried women in our samples. As an additional sensitivity test, a second imputation approach takes the average of each characteristic across the set of possible matches for each unmarried woman. We then use these averages to estimate the characteristics of each synthetic spouse. This is a conventional form of cell mean imputation (see Van Buuren, 2018).

Once we have estimated the synthetic spouse of each of the unmarried women in our sample (aged 25-45), we compare the characteristics of the synthetic spouses with those of actual unmarried men in our sample. We group our data into bins based on age (3-year age categories), race, ethnicity, education (i.e., within 2 years), income (in categories based on increments of $5,000), employment and military veteran status. We then randomly assign unmarried men in each bin to a synthetic spouse, if one exists. This creates a one-to-one matching between the synthetic spouses and actual unmarried men. Unlike most previous studies of marital homogamy, a distinctive feature of our approach is that we account for local opportunity structures by further requiring exact matches of synthetic spouses to real single men on Public Use Microdata Areas (PUMAs) of residence (for exceptions, see Choi & Tienda, 2017; Qian et al., 2018). This process results in a set of unmarried men successfully matched to synthetic spouses, a set of synthetic spouses who have no match, and a set of unmarried men who have no match. We then use information about whether an observation successfully matched to estimate a regression of match probability on the characteristics of unmarried women aged 25 to 45. This provides evidence about which types of characteristics have either an excess demand or a supply shortage in the marriage market.

RESULTS

Baseline Estimates of Marital Mismatch

The first two columns of Table 2 provide our initial hot-deck estimate of the mismatch between the synthetic spouses of the unmarried women (or the characteristics of men these women would likely marry if, in fact, they married) and the actual unmarried men that are available for them to marry. The synthetic spouses have an average income that is about 55% higher ($53,000 vs $35,000), are 26% more likely to be employed (87% vs 70%), and are 18% more likely to have a college degree (29% vs 25%) than the actual unmarried men that are available in the United States. These estimates suggest large differences in the demand and supply of unmarried men with certain characteristics.

(Table 2 about here)

In Figures 1 and 2, we overlay the distribution of age, income, education, and race between the synthetic spouse and the actual unmarried men. Figure 1 is based on hot deck imputation while Figure 2 is based on mean imputation. The locations along the distribution where the shaded bars are higher indicate shortages of unmarried men with specific characteristics. The results in these figures indicate the mismatch for each characteristic separately.

(Figures 1 and 2 about here)

Both Figures 1 and 2 clearly highlight large income- and education-based mismatches in the marriage market. Specifically, there is an excess supply of men with incomes less than $20,000 (with a shortage of men with incomes greater than $40,000) as well as a mismatch in education—too many men have only a high school degree and too few have a college or graduate degree. However, there is evidence that fathers that marry their child's mother experience an increase in income (Killewald 2013). To the extent that this happens, our estimates of the shortage of higher-earning men may be a slight overestimation, but cannot fully explain the magnitude of the shortage.

In contrast to these estimates, the racial distribution is well-matched, although with possible oversupply of unmarried black men, which is a pattern consistent with Wilson's "marriageable male" hypotheses. Because less educated racial and ethnic minorities are the most likely to be incarcerated, this well-matched racial distribution would indicate that the effects of mass-incarceration of blacks on the overall marriage market are insignificant, similar to what is found by Lopoo & Western (2005).

Multidimensional Matching in the Marriage Market

The results in Tables 3 show how women's sociodemographic characteristics *jointly* determine whether they experience a demographic shortfall of unmarried men—those with a demographically-suitable bundle of characteristics. Specifically, we create an indicator for whether synthetic spouses actually match the observed pool of unmarried men. We interpret this as a measure of the ease with which unmarried women are likely to find a suitable marital match. The variables in our imputation models and matching exercise include the aforementioned socioeconomic characteristics of the unmarried woman (see methods section). For ease of exposition, we multiply the coefficients and standard errors by 100 so that they each represent the percentage point change in the probability of having a match in the pool of unmarried men. We run the imputation models separately for three types of matches: matches nation-wide, matches within state and matches with PUMA.

(Table 3 about here)

The overall results in Table 3 indicate, on the one hand, that younger women and less-educated women are more likely to find demographically-suitable matches or potential marital partners available to them. On the other hand, this result also means that older and highly-educated women are especially likely to face shortages of marital partners. This is consistent with other related empirical evidence that sex-ratio imbalances increase with women's age and that the gender reversal in educational attainment has had implications for educational assortative mating (See Lichter and Qian, 2019; Van Bavel et al., 2018). Race also matters. For example, within states, black women are 15.01 percentage points less likely to have a suitable match. Asian women are 3.50 percentage points less likely to have a match. The difficulty in finding a match is larger within PUMAs than within states, especially among Asians (b = -27.23).

Indeed, whether we consider national, state, or local areas as marriage markets clearly matters in our matching exercise. This is to be expected (Brien, 1998). It is plausible—even likely—that some underlying heterogeneity exists across geographic areas in women's ability to find suitable matches. The pool of potential marital partners is larger and more heterogeneous at the national and state levels than at the local-area levels. By requiring marital matches to take place within the same PUMA (Column 3, Table 3), we are in effect accounting for population heterogeneity, i.e., we hold places constant (by looking at matching *within* specific places), which leads to demonstrable differences in the magnitudes of several of the estimates. For example, a 10% increase in a woman's age is associated with a 2.42 percentage point decrease in her likelihood of finding a match nationwide; but when we require that the match be within the same

Public Use Microdata Area (PUMA), the same 10% increase in age correlates with a 15.32 percentage point decrease in likelihood that she finds a match.

In Table 4, we present analysis that applies the same empirical specification used in column 3 of Table 3 (i.e., the within PUMA specification), but disaggregates the analysis by race (columns 1-2), education (columns 3-4), and income (columns 5-7) of the woman. These estimates provide several general conclusions, regardless of specification. For example, older women on average are much less likely find a suitable marital match (within PUMAs). This is especially true among women who are highly educated (column 3, Table 4). A 10% increase in age in women with a college degree is associated with a 24.48 percentage point decrease in the likelihood of a suitable match. In contrast, age matters much less among the least education women—those with a high school degree or less, who have only a 4.47 percentage point decrease in finding a match. One implication is that delaying marriage, for whatever reason but perhaps especially if pursuing college degrees, has the effect of reducing women's local-area access to a synthetic marital partner. One substantive implication is that this may have created demographic pressure for more heterogamous marriages among highly educated women (Qian, 2017).

(Table 4 about here)

Another general conclusion is that both low and high SES women face the largest deficits in the availability of synthetic or suitable male partners (columns 3-7, Table 4). This is indicated by the statistically significant and negative coefficients in virtually every cell of Table 4 (columns 3-7). These negative estimates represent deviations from the reference categories in our models—women with some college education or with incomes of more than $20,000 but less than $40,000. These women "in the middle" evidently face the fewest demographic constraints in local marriage markets.

Finally, it is also the case that minorities—black, Asian, and other racial minority women, including Hispanics of any race—are significantly less likely to find suitable partners, regardless of education or income (columns 3-7, Table 4). When we compare black and white women separately (columns 1-2, Table 4), we find considerable similarity in the direction but not the magnitude of sociodemographic factors associated with women's access to synthetic spouses. The largest differences are with respect to unemployment and labor force nonparticipation. Specifically, white unmarried women—those who are detached from the labor force—are much more likely than their black unmarried counterparts to face shortages of potential partners. These white women are about one-third less likely to find a match than their employed white counterparts. For black women, these figures are much lower.

To further explore possible race differences, we also included interaction terms for Black*College Graduate and Black*Income>=100,000 in our models to examine whether demographic mismatches are significantly larger for black than white women with a college degree or with high income (data not shown). We found that black woman with college degrees are less likely (about 3 percentage points) to be matched than similarly educated white women. Racial differences are even larger when we consider high-income women. For black women with incomes of $100,000 or more, the difference from similar white women is about 15 percentage points, a result that clearly highlights deficits in suitable partners for these high SES black women.

DISCUSSION AND CONCLUSION

Claims that today's unmarried women face serious shortages of "good men" to marry are commonplace in the family sciences literature (Kreager, Cavanagh, Yen, & Yu, 2014; Raley & Bratter, 2004). Previous studies have typically focused narrowly on sex ratio imbalances—on the question of whether low-income or minority women face deficits in men available to marry (Blau, Kahn, & Waldfogel, 2000; Lichter et al., 1992). Our analysis, based on 10 years of data from the *American Community Survey*, provides a direct test of such claims based on the national and sub-national availability of men that are typically matched to women with a specific characteristics or skills.

Our analyses provide clear evidence of an excess supply of men with low income and education and, conversely, shortages of economically-attractive unmarried men (with at least a Bachelor's degree and higher levels of income) for women to marry. One implication is that promoting good jobs may ultimately be the best marriage promotion policy rather than marriage education courses that teach new relationship skills. Of course, other policy efforts aimed at securing women's economic independence (i.e., equal pay legislation) are also important in the case of single mothers, who often face constraints on marital search behavior and have limited prospects for "marrying up" (Bzostyek, McLanahan, & Carlson, 2012; Lichter et al., 2003). Our estimates of marriage market disequilibria are instructive, especially at a time when marriage is sometimes viewed as an economic panacea (for discussion, see Lichter Batson, & Brown, 2004). In the case of unmarried minority women, for example, shortages of highly educated unmarried men also impose serious constraints on the marital search process.

Black women, for example, are about 17 percentage points less likely than white women to have a match in their local marriage market area (PUMA).

Our findings also make the case that highly-educated white women face shortages of marriageable men. For highly-educated women, the marriage market implications of new gender imbalances in educational achievement seem increasingly clear (Buchmann & DiPrete, 2006). They will either increasingly remain unmarried or, alternatively, conventional patterns of marital educational hypergamy (i.e., women marrying up in education) may give way to educational hypogamy as women adapt to deficits in the pool of highly-educated men (Qian, 2017). Previous studies, although now dated, suggest that most unmarried women choose to remain single rather than to "marry down" or non-assortatively (Lewis & Oppenheimer, 2000; Lichter, Anderson, & Hayward, 1995). In today's highly competitive marriage market, however, this is an issue worth revisiting (Schwartz and Han, 2014; Qian, 2017).

This study is not without some limitation. For example, we acknowledge that there are unmeasurable selection factors that may differentiate married women from unmarried women. Our results should therefore be interpreted to indicate what the marriage market should look like if all women were to have a plausible match, regardless of their inclination towards marriage. It is also worth noting that selection is unlikely to be homogeneous across demographic groups; indeed, this may explain why we find that higher-educated women experience higher marriage rates, even though they have less potential matches. The implication is that they may increasingly "marry down" in education (Qian, 2017).

A large share of adolescents and young adults today expect to marry and this is little changed from previous generations (Anderson, 2016; Manning et al., 2007). This makes clear that most women—black or white, rich or poor, highly-educated or uneducated—have "high hopes" for marriage, yet growing shares of women today either delay marriage or remain unmarried altogether (Gibson et al., 2005; Lichter et al., 2004). Our study uncovers the demographic reality of large deficits in the supply of men who are suited or well-matched for today's unmarried women. If nothing else, our empirical results indicate that the U.S. marriage market is currently in disequilibria. The supply of unmarried men is out of demographic balance with the demand for marriageable men among America's currently unmarried women. Whether this is new or different from past generations is unclear, as is the question of whether marriage market mismatch is fully or partly responsible for the ongoing "retreat from marriage". What is clear is that the characteristics of potential spouses—male and female—have become more diverse over time with rising educational levels among women, increasing racial diversity, and new patterns of income and educational inequality.

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Table 1. Summary Statistics

	Married Women	Married Men	Unmarried Women	Unmarried Men
Employed	69.54	90.95	74.62	69.55
Unemployed	3.79	3.65	7.05	8.28
Not in the Labor Force	26.66	5.41	18.33	22.17
Personal Income	33,785	70,353	32,332	34,552
	(44,591)	(76,479)	(36,028)	(43,854)
Percent White	79.39	79.87	67.15	69.93
Percent Black	5.47	6.24	18.47	15.16
Percent Hispanic	15.56	15.20	16.59	17.34
Age	36.50	38.90	33.72	33.32
	(6.65)	(7.49)	(6.31)	(6.20)
High School Graduate	91.96	89.98	89.59	85.08
Some College	73.09	66.37	66.92	54.00
College Graduate	42.04	37.08	33.35	24.89
N	2,389,035	2,389,035	1,512,154	1,711,805

Notes: Unmarried individuals are between the ages of 25 and 45. All married individuals are included for which at least one spouse is 25-45.

Table 2. Comparison of Synthetic Spouses and Unmarried Men

	Unmarried Men	Synthetic Spouse of Unmarried Women	Difference in Means	Percentage Difference
Employed	69.55	87.30	-17.75	25.52%
Unemployed	8.28	5.45	2.83	34.17%
Not in the Labor Force	22.17	7.25	14.92	67.31%
Personal Income	34,552	52,757	-18,205	52.69%
	(43,854)	(56,354)		
Percent White	69.93	69.44	0.49	0.70%
Percent Black	15.16	18.26	-3.10	20.43%
Percent Hispanic	17.34	15.42	1.92	11.06%
Age	33.32	36.29	-2.97	8.91%
	(6.20)	(8.48)		
High School Graduate	85.08	88.96	-3.88	4.56%
Some College	54.00	60.96	-6.96	12.88%
College Graduate	24.89	29.43	-4.54	18.26%
N	1,711,805	1,497,915		

Notes: Unmarried men are between 25 and 45. Synthetic spouses are those of unmarried women aged 25-45. The percentage difference is calculated as follows: (unmarried man mean - synthetic spouse mean)/(unmarried man mean) x 100. All differences between unmarried men and synthetic spouses of unmarried women are statistically significant at the .01 level.

Table 3. Characteristics that predict whether an unmarried woman is likely to have a potential match

	All Matches	Matches Within State	Matches Within PUMA
Log Personal Income	-0.32***	-0.12***	-1.07***
	(0.04)	(0.04)	(0.04)
Log Age	-2.42***	-8.10***	-15.37***
	(0.20)	(0.21)	(0.21)
Black	-15.01***	-14.37***	-17.19***
	(0.11)	(0.11)	(0.10)
Asian	-3.50***	-8.73***	-27.23***
	(0.16)	(0.18)	(0.15)
Other Race	-1.96***	-5.51***	-10.62***
	(0.13)	(0.14)	(0.14)
Hispanic	-0.30***	0.18	-13.80***
	(0.10)	(0.11)	(0.11)
Some College	-3.47***	-3.54***	-4.70***
	(0.09)	(0.09)	(0.10)
College Graduate	-1.54***	-2.77***	-7.60***
	(0.09)	(0.09)	(0.10)
Not in the Labor Force	-39.96***	-36.09***	-25.71***
	(0.13)	(0.13)	(0.12)
Unemployed	-40.44***	-36.36***	-25.74***
	(0.17)	(0.17)	(0.15)
Mean of Matched	66.35	60.21	39.67
N		1,511,601	

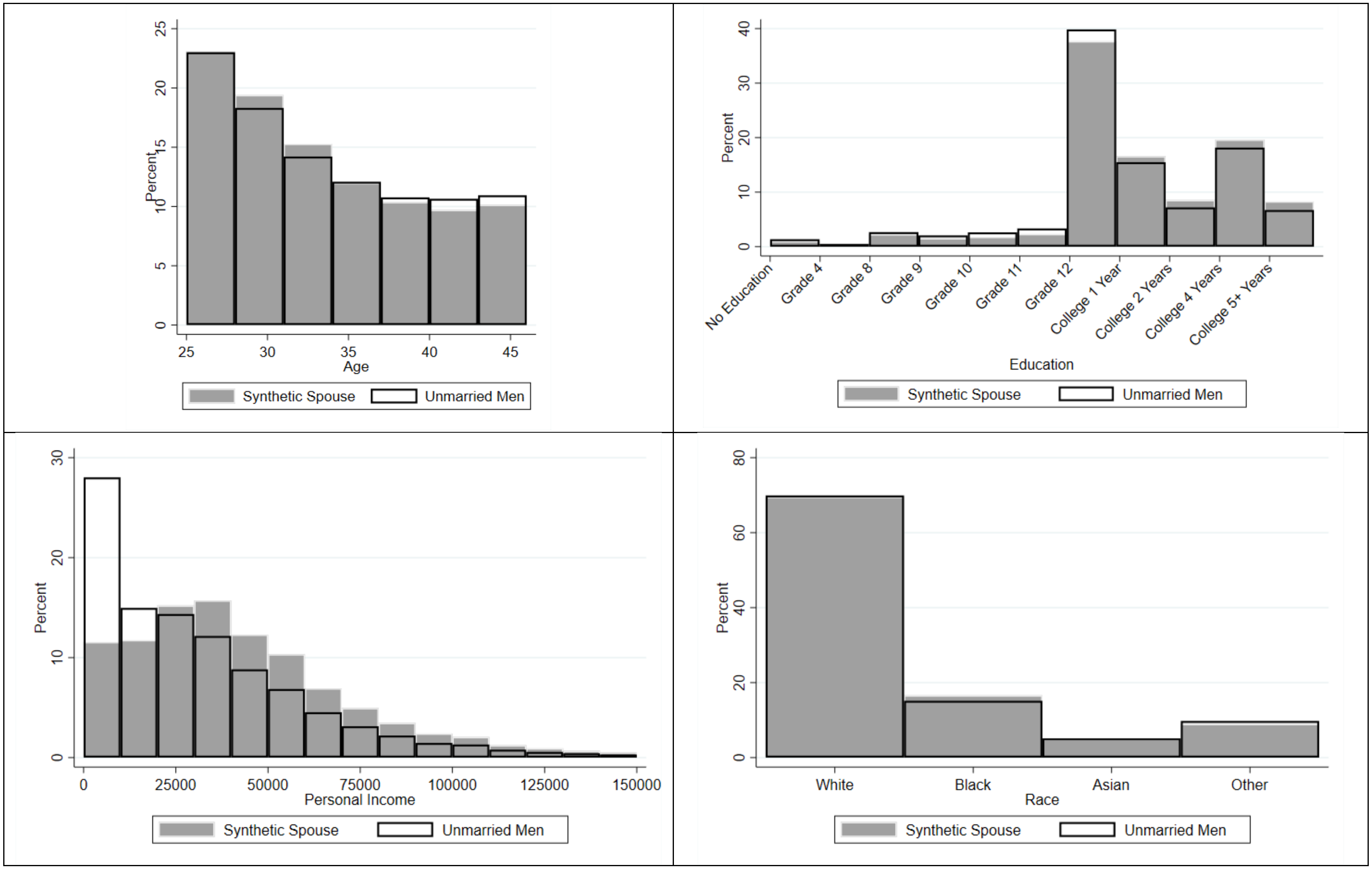
*Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Excluded groups are White, high school or less education, and employed. Women are aged 25-45. Matches were within two years of education, income within $5,000, and age within three years. Coefficients and standard errors are multiplied by 100. As these are samples, mean of match does not indicate the probability a woman has a unique match in reality, but rather it is an indicator for the ease of finding a match.*

Table 4. Characteristics that predict whether an unmarried woman is likely to have a potential match within PUMA

	Black	White	College Degree	HS or Less Education	Income <=20,000	Income >=40,000	Income >=100,000
Log Personal Income	-0.74***	-1.35***	-2.57***	1.47***	-9.87***	-10.41***	-0.32
	(0.09)	(0.05)	(0.07)	(0.11)	(0.11)	(0.19)	(0.53)
Log Age	-14.50***	-14.04***	-24.48***	-4.47***	-16.44***	-15.01***	-13.60***
	(0.48)	(0.26)	(0.36)	(0.65)	(0.29)	(0.41)	(1.22)
Black	-	-	-19.01***	-13.22***	-22.61***	-22.00***	-18.25***
			(0.19)	(0.32)	(0.14)	(0.19)	(0.55)
Asian	-	-	-23.93***	-37.58***	-27.19***	-24.61***	-18.41***
			(0.20)	(0.51)	(0.20)	(0.24)	(0.52)
Other Race	-	-	-11.77***	-9.96***	-12.87***	-12.86***	-15.90***
			(0.28)	(0.31)	(0.20)	(0.29)	(0.76)
Hispanic	-7.26***	-13.94***	-9.81***	-17.00***	-13.74***	-12.45***	-10.03***
	(0.49)	(0.13)	(0.23)	(0.28)	(0.16)	(0.23)	(0.68)
Some College	-4.64***	-5.25***	-	-	-2.04***	-0.78***	2.13**
	(0.20)	(0.12)			(0.15)	(0.26)	(0.95)
College Graduate	-6.63***	-8.63***	-	-	-4.43***	-3.39***	-0.47
	(0.22)	(0.12)			(0.12)	(0.17)	(0.60)
Not in the Labor Force	-7.06***	-33.11***	-35.87***	-13.40***	-30.47***	-29.91***	-25.50***
	(0.28)	(0.16)	(0.25)	(0.31)	(0.26)	(0.38)	(0.74)
Unemployed	-8.73***	-34.44***	-33.14***	-14.82***	-29.23***	-29.23***	-25.14***
	(0.30)	(0.19)	(0.27)	(0.41)	(0.27)	(0.40)	(0.84)
Mean of Matched	29.57	45.48	37.78	34.48	42.53	38.46	28.31
N	279,186	1,015,041	504,098	157,423	873,166	445,889	53,341

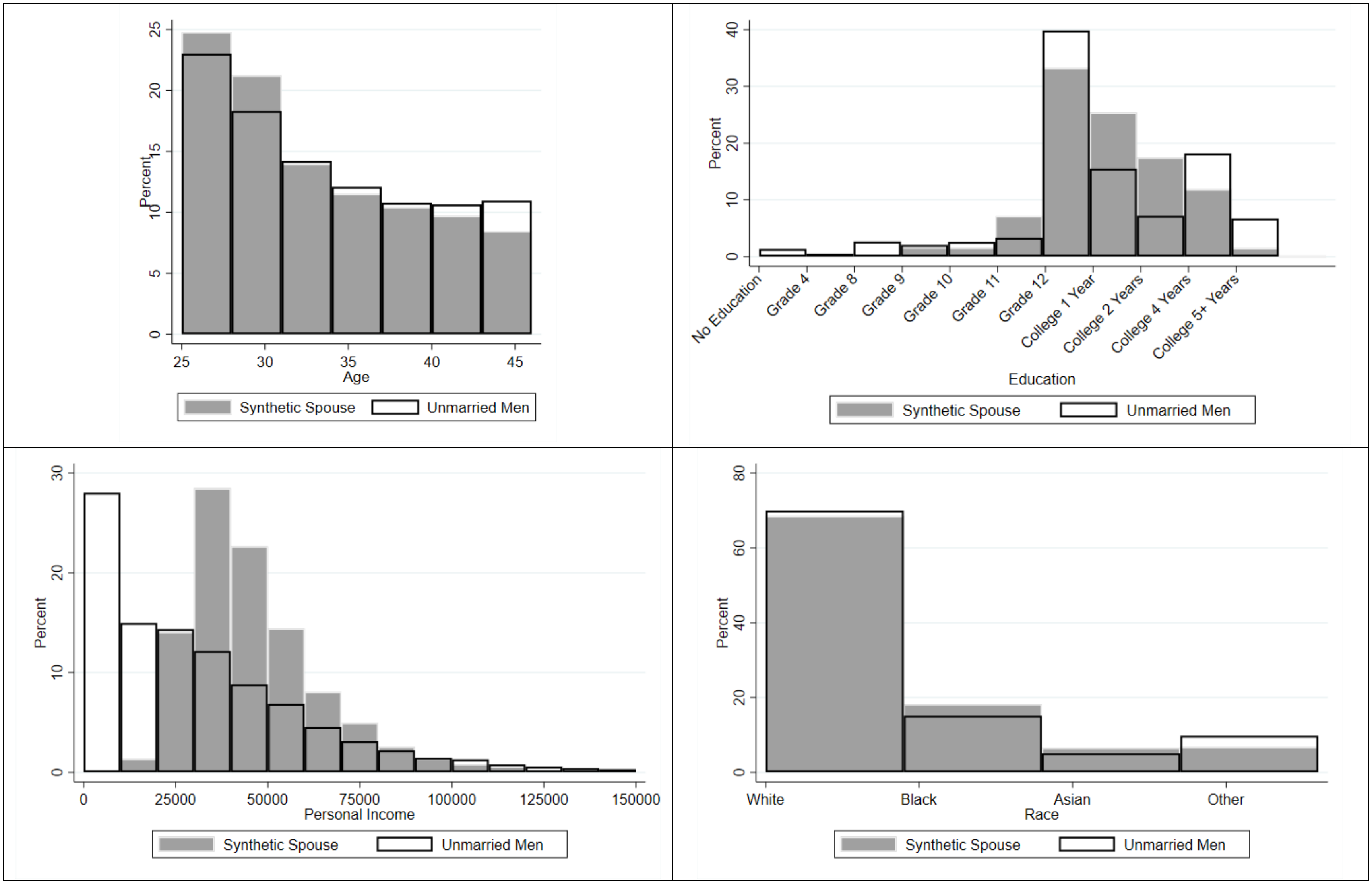
*Notes: Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. This table is based on within PUMA matches (column 3 of Table 2) but splits the sample based on the characteristic of the unmarried women. Women are aged 25-45. Coefficients and standard errors are multiplied by 100.*

Figure 1. Comparison of distribution of synthetic spouses and actual unmarried men using hot deck imputation.



Notes: Imputation was run for all individuals. Married couples used in imputation were married in the previous five years. Presented are restrictions of theoretical spouses aged 25-45 and unmarried men aged 25-45.

Figure 2. Comparison of distribution of synthetic spouses and actual unmarried men using mean imputation.



*Notes**:** Imputation was run for all individuals. Presented are restrictions of theoretical spouses aged 25-45 and unmarried men aged 25-45.*

Original Research Communications

Can changing the position of online menu items increase selection of fruit and vegetable snacks? A cluster randomized trial within an online canteen ordering system in Australian primary schools

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ABSTRACT

Background: Manipulating the position of food items within the physical food environment has consistently been found to influence item selection. However, the extent to which this strategy is effective in an online food environment is unknown.

Objective: This study investigated whether an intervention to position fruit and vegetable snack items as the first and last menu items in an online school canteen ordering system increased the selection of those items. It was hypothesized that at follow-up, a higher proportion of online lunch orders in intervention schools would contain the target items (fruit and vegetable snacks) in comparison to control schools.

Design: Six primary schools in New South Wales, Australia, were recruited to a clustered randomized controlled trial conducted over an 8-wk period. Intervention schools received a redesigned menu where the target items were positioned first and last on the online menu. Control schools received no change to their online menu.

Results: During the baseline period 1938 students (1203 intervention, 735 control) placed at least one online lunch order and were included in the study, with 16,109 orders placed throughout the study. There was no significant difference between groups over time in the proportion of orders that contained a "Fruit and Veggie Snack" item (OR = 1.136 [95% CI: 0.791, 1.632] $P = 0.490$).

Conclusions: Evidence from this large trial with robust study design and objectively collected data suggests that positioning fruit and vegetable snack items first and last within an online canteen menu does not increase the selection of these items. Further research is warranted to confirm this finding with other target menu items (e.g., treats) and across other purchasing contexts and online food ordering platforms. This trial was registered at the Australian New Zealand Clinical Trials Registry, http://www.anzctr.org.au/ as ACTRN12616001520426. *Am J Clin Nutr* 2019;00:1–9.

Keywords: RCT, choice architecture, nudging, position, online, canteens, children, school, intervention

Introduction

Inadequate fruit and vegetable consumption is a recognized risk factor for cardiovascular disease and some cancers, and is estimated to be responsible for 2.6 million deaths per year (1). Evidence suggests that increased fruit and vegetable consumption in childhood may also help reduce the risk of adult chronic disease (2–4). Internationally, many children have diets that are not consistent with dietary guidelines recommended for healthy growth and development (5), with the large majority of children in the US, the UK, and Australia failing to eat the recommended daily servings of fruit and vegetables (6–8). Consequently, there is need to investigate public health interventions to improve childhood diet.

Choice architecture interventions (9, 10) alter the environments in which decisions are made in order to produce a predictable change in behavior (9). A range of choice architecture strategies have been found to be effective in changing dietary behaviors (9, 10). One strategy that has been widely

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investigated is varying the position of available food items, such as manipulating their proximity, visibility, arrangement, or order (11). This strategy is thought to work by increasing the salience of repositioned items (12). A 2016 systematic review concluded that positioning strategies have been consistently shown to influence the selection and consumption of targeted foods (11). Much of this evidence, however, has been generated from laboratory-based studies which may not be applicable to real-world purchasing environments.

In recent years there have been significant shifts in the approach to food purchasing with the rapid increase in online food ordering. For example, online grocery and food catering sales have demonstrated substantial growth (13), and there is a large and growing customer base using food delivery and takeaway apps (14). This trend is also influencing food purchasing within schools. Online canteens, where parents go online to access their school's canteen menu and order and pay for their child's lunch, are increasingly popular in Australian schools (15). Given the rise of online food environments, research is required with regards to the impact of choice architecture strategies in these contexts.

Early research into the arrangement and presentation of items within the online environment explored the impact of positioning by manipulating links within webpages and emails. These serial positioning studies indicated that the higher a link was positioned within a list, the higher the number of clicks it received (16, 17) providing evidence of a primacy effect (18). Subsequent studies into online positioning have also provided evidence of a 'recency' effect, whereby links at both the beginning and end of an online list are clicked more frequently than items located in the middle of the list (19). This has been demonstrated with lists of hotels within a webpage (20) and links displayed on a restaurant's website (21). Both primacy and recency effects are theorized to be the result of improvements in short-term memory (21, 22), aligning with studies of the physical food environment which postulate that repositioning foods can increase their salience (11) and their strength within short-term memory.

However, there have been few attempts to test the effect of positioning strategies to encourage the selection of healthier foods in online environments. A 2012 study investigated the impact of manipulating the display of foods in a 'virtual' university canteen (23). The study found no difference in the sales of healthy snacks when they were displayed on the top shelf, relative to the bottom shelf (23). In contrast, a previous study by the authors found a multi-component intervention that included a positioning strategy to be effective when delivered via an online canteen ordering system (24). Given the trend toward online food purchasing and the limited research to date investigating the effects, in particular, of positioning strategies in this context, rigorous research is required to address this evidence gap.

The objective of the current study was to determine whether the positioning of fruit and vegetable snack items first and last on an online menu increases selection of those items, as measured by the proportion of lunch orders that include those items. It was hypothesized that the proportion of online student lunch orders that contain at least one target item (fruit and vegetable snack) would be higher in intervention schools relative to control schools at follow-up.

Methods

Design

A parallel group clustered randomized controlled trial was conducted over an 8-wk period. Six primary schools in New South Wales (NSW), Australia, that were already utilizing an online ordering system in their school canteen were randomized to receive either a redesigned menu, so that the fruit and vegetable snack items were positioned to appear first and last on the menu (intervention), or to receive no change to the design of their online menu (control). A clustered design was required as it was not possible to randomly assign individual students or order occasions within the online ordering system. Instead, schools were randomly assigned to the intervention or control conditions. The conduct of the trial was approved by the Human Research Ethics Committees of Hunter New England Health (06/07/26/4.04), The University of Newcastle (H-2008–0343), and the New South Wales Department of Education and Communities (State Education Research Application Process 2,012,277). The study was registered with the Australian New Zealand Clinical Trials Registry, after recruitment had commenced but prior to intervention delivery (ACTRN12616001520426).

Recruitment

Schools (clusters).

A provider of a school canteen online ordering system (henceforth 'the Provider') supplied the researchers with a list of all NSW government schools they serviced. To be eligible, schools were required to have been using the online lunch ordering system for a minimum of 6 mo, and processing a minimum of 50 student lunch orders per month. Special purpose schools catering for students with special needs (e.g., juvenile justice or hospitalized children) were ineligible due to potential differences in food provision within these settings. A sampling approach was adopted whereby the list of schools was ordered from the largest to smallest number of online canteen lunch orders placed per month, and schools were contacted in order until the required sample of schools consented to participate. A research assistant mailed study information and a consent form to the Principal and Canteen Manager at each school, and followed-up no sooner than 2 wk later via telephone. The first 6 schools to return a consent form indicating Principal consent were included in the trial.

Individuals.

All users (parents and students) of the online ordering system that placed at least one student lunch order during the baseline period were eligible for inclusion in the trial.

Randomization

After all schools had been recruited to the trial, a statistician used an Excel computer program to generate a random sequence and, using simple randomization, allocate consenting schools (clusters) to the intervention or control conditions in a 1:1 ratio. This trial was run as an open trial, due to the difficulty in masking the redesigned menu changes among intervention schools. However, students and parents did not receive formal notification of the changes made to their online canteen menu or the purpose of such menu modifications.

Intervention

Context.

Most Australian schools have a school canteen that sells food and drink at recess and lunch breaks throughout the school day. Students can either bring food from home or purchase food from the school canteen. Typically, canteen foods that are to be consumed in the lunch break are pre-ordered, whereas snack foods and drinks are purchased over the counter. Snack foods (including fruit and vegetable snacks) are usually available at both recess and lunch, with hot food and main meals (e.g., sandwiches, wraps, rolls, and salads) only available at lunchtime. Between 39% (Kindergarten) and 57% (Year 6) of NSW primary school students purchase their lunch from the school canteen once or more per week (25).

Development and rationale.

The intervention development was guided by evidence from peer-reviewed literature. In the absence of randomized controlled trials directly comparing alternative versions of online menus, evidence from similar contexts suggests that repositioning healthy menu items can significantly increase their selection. Studies indicate this effect is found both in the physical environment (26) and on paper-based menus, with items first and last in an array being selected up to twice as often as those in the middle (27). Furthermore, intervention development also involved consideration of strategies that were amendable to scale-up, feasible to implement, acceptable to key stakeholders (24), and likely to influence the behavior of parents. Pilot work included a telephone survey of 47 parents of primary-school aged children asking how likely it was that *"Healthier menu items being more prominent than unhealthy options"* would influence what they bought from the school canteen for their children. The survey found that 87% of parents indicated that this strategy would be 'likely' or 'very likely' to influence what they purchased.

Overview.

Intervention schools participating in this trial had their online canteen menu redesigned so that the fruit and vegetable snack items were positioned first and last on the menu.

The online canteen system.

Online school canteen ordering systems enable students' school lunches to be selected and paid for via the web. Lunch orders typically consist of 2 to 3 items including a main meal item such as a sandwich or hot food item, and a snack and/or drink item. The online ordering system displays the school's canteen menu in a vertical list so that users are required to scroll down to see all available items. Items are typically ordered into categories such as "Hot Food", "Sandwiches, Wraps and Rolls", or "Snacks", with similar items grouped together within categories. However, the particular arrangement of items into categories, the order of items within a category, the order categories are displayed, and the name each category is given, is determined by the Canteen Manager in each school. The Canteen Manager can also select from a range of images of food and drink items to display beside each food category.

Intervention strategies.

A dietitian with over 6 years' of experience working with school canteens reviewed each school's canteen menu and identified target items. Target items included any fruit or vegetable menu items (fresh, frozen, tinned, or dried) that children could consume as a snack. Main meal items that incorporated fruit and/or vegetables (e.g., a hamburger with salad) were not included as targets as their proportion of fruit and vegetable content varied widely and it was not possible to ensure that they represented a healthy choice or that they were compliant with the statewide government canteen guidelines for healthy foods (Fresh Tastes @ School) (28). The menus of intervention schools were then redesigned to ensure target items were listed first and last on the online menu (27). In order to execute this change, the target items were grouped together in a single category titled "Fruit and Veggie Snacks", which was displayed in two places; first and last categories on the online menu. Within the "Fruit and Veggie Snacks" category, items were listed based on the following order: fresh fruit – whole; fresh fruit – cut up (e.g., fruit salads, kebabs); frozen fruit; tinned fruit; dried fruit; fruit with accompaniments (e.g., yogurt); fresh salad vegetables; cooked vegetables; vegetables with accompaniments (e.g., dip, salsa, cheese). As is the convention within the online system, an image (of an apple) was displayed beside the new category name ("Fruit & Veggie Snacks"). If the apple image was already being used on the school's menu (e.g., next to a "Snack foods" category) then it was removed from this other category and replaced with another relevant symbol (e.g., popcorn); this occurred in one intervention school only. All other menu design, labeling, and positioning remained unchanged (see **Figure 1**).

The Provider executed the changes within the online system. Changes were made on a Friday afternoon to ensure that users placing lunch orders over the weekend for the following week would be exposed to the intervention. A research assistant then checked the online menu of each intervention school to ensure the changes had been implemented correctly. The intervention was implemented in November 2016 and ran for a 4-wk period (weeks 5–8 of the fourth term of the school year).

Intervention fidelity.

The research assistant reviewed the online menus of all intervention schools each Monday morning throughout the intervention period to ensure that the intervention was implemented as intended. The research assistant checked that no new target items had been added, and if they had been, ensured they were incorporated according to intervention protocol.

Control

No changes were made to the online menus of control schools.

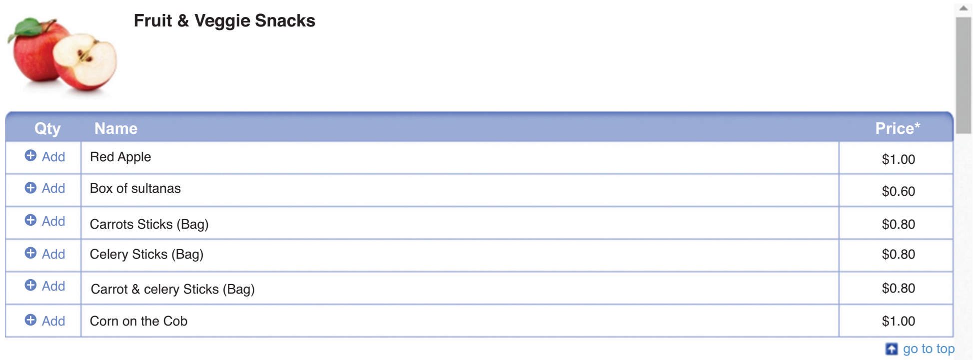

FIGURE 1 Screenshot of the "Fruit & Veggie Snacks" category in an intervention school.

Data collection

Baseline data was collected in the 4-wk period prior to intervention commencement (October 2016). Follow-up data was collected in the 4-wk period following intervention commencement (November 2016). Only lunch orders that had been placed after the intervention strategies implementation were included in the data set. Recurrent lunch orders (i.e., lunch orders that are placed ahead of time, to be delivered on multiple, recurring occasions in the future e.g., every Friday for the remainder of the term) were excluded from analysis as it was not possible to ensure that the user was exposed to the intervention.

Measures.

The online ordering system automatically records all sales data and revenue from lunchtime orders processed through the system. It also records the grade level of the student for whom the order is placed. At the conclusion of the trial, the Provider supplied the data to the researchers in an Excel file. The school enrollment data and postcode were accessed via the MySchool website (a government website containing information about every Australian school). The school postcode was used to classify the rurality (29) and socio-economic status (30) of the school locality.

Outcomes

Primary outcome (orders): the proportion of all online lunch orders that contained at least one target item (fruit or vegetable snack food).

Secondary outcome (items): the proportion of all individual items within all online lunch orders that are target items (fruit or vegetable snack foods).

Secondary outcome (revenue): the average weekly canteen revenue from online lunch order sales was analyzed to determine if the intervention impacted overall online canteen sales (adverse outcome). Canteen revenue for each school is automatically recorded by the online ordering system.

Sample size and analysis

In order to detect a 10% increase in the proportion of lunch orders that contained a target item (with 80% power, at the 0.05 significance level), assuming an average of 300 orders per school placed at baseline, with target items present in 10% of control group orders, and an ICC of 0.01, it was determined that 3 schools per group (with equal clusters) would be required.

A statistician used SAS version 9.3 statistical software (SAS Institute Inc.) to analyze the data. An intention to treat approach was adopted to compare between group differences over time using a group by time interaction effect within a linear mixed model, adjusted for both clustering at the school level and student level (i.e., multiple orders placed by the same student). The main analysis used all participants who placed an order at least once during the baseline period. A sensitivity analysis was then undertaken for the primary outcome, whereby missing data at follow-up was imputed (31) in cases where an order was placed for a student during the baseline, but not the follow-up period. To aid interpretation of the results, an exploratory analysis was undertaken to identify the mean number of fruit and vegetable items purchased per order, to provide an indication of the intervention effect at the level of the individual student. This analysis was not preregistered. Average weekly revenue from online lunch order purchases was assessed using linear mixed regression, with 4 time points for each school entered into the model for each period (i.e., 4 wk of data at baseline and 4 wk of data at follow-up across the 6 schools). Analysis used the Kenward-Roger approximation to account for the low number of schools (clusters).

Results

The flow of participants through the trial is shown in **Figure 2**. Recruitment continued until 6 schools consented to participate in the trial. The characteristics of participating schools are detailed in **Table 1**.

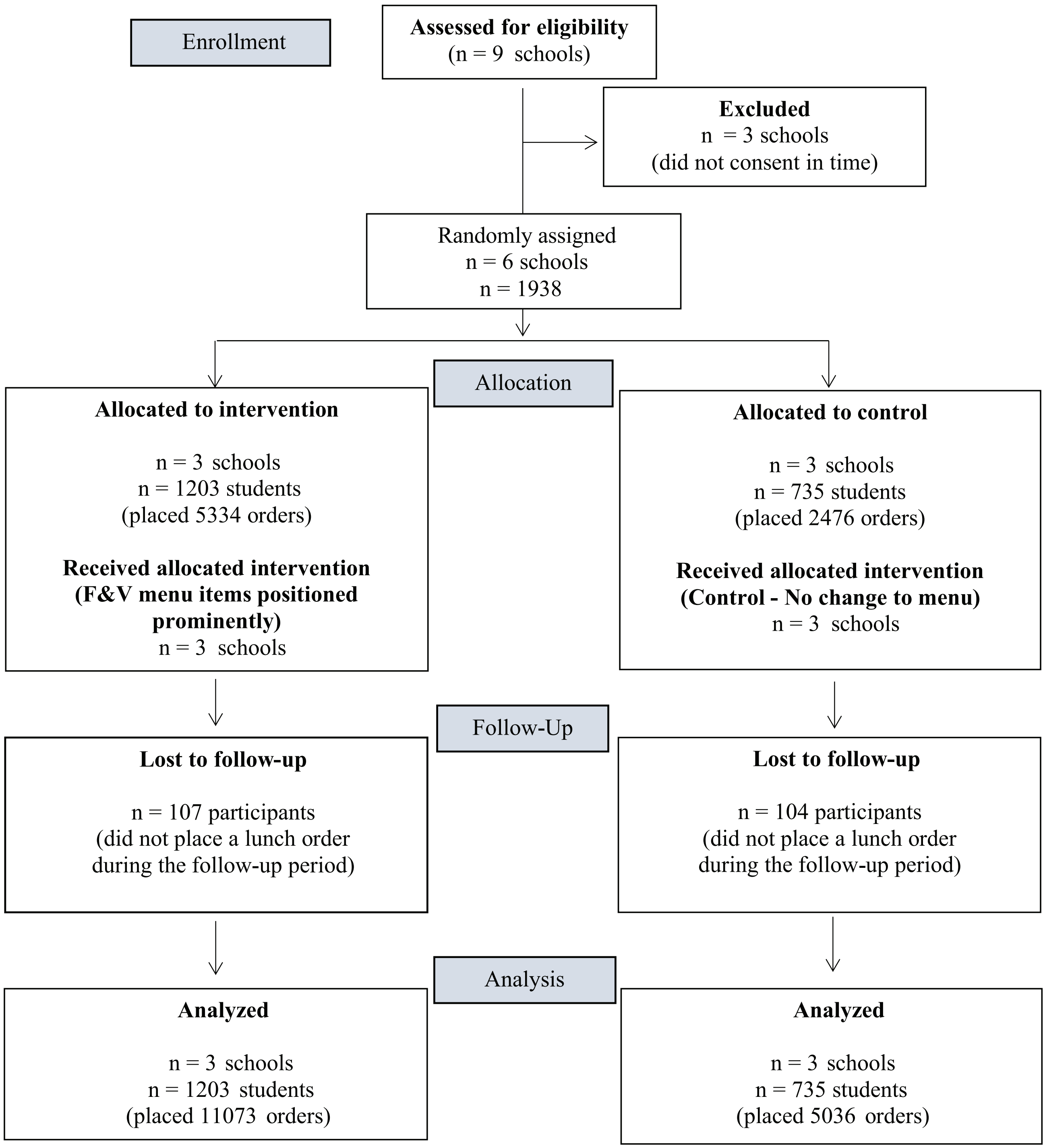

FIGURE 2 CONSORT diagram showing progression through the trial. Two schools were initially recruited to another online canteen trial (24), but were subsequently discovered to be ineligible for that trial. They were then invited to participate in the current trial, and met all eligibility criteria.

Schools

The average number of student enrollments was higher in intervention schools (n = 609), compared with control schools (n = 515). All participating schools were located in "Major Cities in Australia", and all but one control school was located in a disadvantaged area.

Menus

At baseline, intervention schools had an average of 7.0 fruit or vegetable snack (target) items per menu, representing an average of 9.1% of all menu items, whereas control schools had an average of 3.3 items per menu, representing an average of 5.1% of all menu items per menu.

TABLE 1 Baseline characteristics of sample

	Intervention M(SD)/%	Control M(SD)/%
Schools (Clusters)	n = 3	n = 3
Size (enrollments)	609 (119)	515 (281)
Rurality (major cities)[1]	3 (100%)	3 (100%)
Socio-economic status (least advantaged)[2]	3 (100%)	2 (67%)
Menus		
Average number of “target” fruit &vegetable items per menu	7.0 (range 6–8)	3.3 (range 3–4)
Average % of menu comprised of target items	9.1% (range 7.7–10.5%)	5.1% (range 4.5–5.6%)
Users	$n = 1{,}203$	$n = 735$
- Kindergarten–Grade 2	573 (47.6%)	334 (45.4%)
- Grade 3–Grade 6	630 (52.4%)	401 (54.6%)
Orders	5,334	2,476
Frequency of ordering		
- Low (less than once a week)	614 (51.0%)	440 (59.9%)
- High (once or more a week)	589 (49.0%)	295 (40.1%)
Items ordered	11,073	5,036

[1]Based on Australian Standard Geographical Classification (ASGC).
[2]Based on Socio-Economic Indexes For Areas (SEIFA) 2011.

Users

The distribution of younger and older students was simila between intervention and control groups (47.6% compared with 45.4% in Kindergarten–Grade 2), however, a higher proportion of students at intervention schools (49.0%) ordered their lunch once or more per week relative to students at control schools (40.1%).

Students

During the 4-wk baseline period, 1,938 students (1203 intervention, 735 control) had at least one online lunch order placed and were included in the trial, with 1096 students in the intervention group and 631 students in the control group having orders placed at both time points.

Orders

At baseline, 7810 orders (5334 intervention and 2476 control) were placed with 16,109 orders placed throughout the entire trial (11,073 intervention and 5036 control). **Table 2** displays the primary and secondary trial outcomes.

The proportion of lunch orders containing at least one target item increased marginally from baseline to follow-up across both intervention (9.24–10.63%) and control groups (4.48–5.23%). There was no significant difference between groups over time (OR = 1.136 [95% CI: 0.791, 1.632] $P = 0.490$). When data was imputed for missing data at follow-up, the results were unchanged (OR = 1.151 [95% CI: 0.804, 1.649] $P = 0.442$).

Results displayed a similar pattern at the item level. Within both the intervention and control groups, there were very small increases in the proportion of target items purchased from baseline to follow-up (intervention: 5.17% to 6.01%; control: 2.27% to 2.64%). However, the between group difference over time was not significant (OR = 1.051 [95% CI: 0.653, 1.618], $P = 0.991$). Post hoc analysis indicated that this corresponded to an average of 0.12 (SD = 0.36) target items per lunch order in intervention schools at follow-up (up from 0.10 items at baseline), and an average of 0.05 (SD = 0.23) target items per lunch order in control schools at follow-up (unchanged from 0.05 items at baseline).

Analysis of the mean weekly revenue indicated that the revenue from online lunch orders was higher in intervention schools and revenue from both groups increased between the baseline and follow-up periods. However, there was no difference between groups over time, indicating that there was no adverse effect of implementing the intervention on the revenue of the online canteen.

Intervention fidelity

A research assistant checked the online menus for intervention schools at the beginning of weeks 2 and 3 of the intervention. The checks revealed that no changes had been made to the menus within this time period. In the online menus of control schools at baseline, all fruit and vegetable snack items appeared in the middle of the menu. Specifically, they were located within either the last or second to last menu categories (labeled as “snacks” or “salads”), or at the middle or beginning of the category.

Discussion

This randomized controlled trial tested whether a positioning intervention, whereby target fruit and vegetable snack items were moved to the first and last positions on an online menu, could increase selection of the these items. It was hypothesized that the intervention would significantly increase the proportion of lunch orders that contained at least one target item. Data from 1938 students across 3 intervention and 3 control schools suggested that the proportion of lunch orders containing target items was not significantly different between groups over time. This was supported by the analysis of secondary outcomes which revealed that the proportion of target items purchased did not significantly differ between intervention and control groups at follow-up. Similarly there was no difference over time in weekly canteen revenue. The findings suggests that as a stand-alone strategy, repositioning of menu items may not be sufficient to increase the selection of fruit and vegetable snacks from an online canteen menu.

There is a substantial body of trial and systematic review evidence supporting the efficacy of manipulating the position of menu items and food or drink offerings within the physical

TABLE 2 Primary and secondary trial outcomes

	INTERVENTION		CONTROL		Odds ratio for time × group interaction (95% CI)	*P* value
	Baseline	Follow-up	Baseline	Follow-up		
% of all lunch orders containing target items (*n*)[1]	9.24% (493)	10.63% (610)	4.48% (111)	5.23% (134)	1.136 (0.791,1.632)	0.490
% of all lunch order items that are target items (*n*)	5.17% (538)	6.01% (678)	2.27% (113)	2.64% (135)	1.051 (0.653,1.618)	0.991
Weekly mean revenue ($Australian dollar)[2]	$2,093	$2,307	$941	$974	$180* (−$16, $376)	0.07

[1]Analysis: linear mixed regression, adjusted for both clustering at the school level and student level.
[2]Analysis: linear mixed regression.
*Relative Mean Difference.

environment (e.g., the display of foods within a cafeteria) to encourage a healthier choice (11). However, to the best of our knowledge, only one study has previously tested the isolated effect of positioning strategies of food selection within a virtual environment. In this study, students were required to choose an item from a virtual array contained within a set of shelves displayed on a computer screen. The position of target items varied within the shelves (top shelf compared with bottom shelf) (23). Consistent with the findings of the current trial, the study showed that intervention did not significantly shift selection behavior. The authors theorized that the manipulation of item position was not extreme enough to alter the participants' access to the target items as the difference between accessing a conveniently versus inconveniently positioned target item was simply a matter of eye movement (23). The manipulation in the current trial was more extreme, given the online canteen menus span multiple screens and required participants to scroll down past the target category to view any other menu items. However, it still may not have been enough to induce an effect beyond other drivers of purchasing behavior including habit, pricing, and availability. In both trials, little effects of the positioning strategies were evident.

In the current trial, the null effect may be in part attributable to the selection of the lunch break as the period under observation or the choice of target items within the purchasing context. For example, fruit and vegetable snacks may be more commonly purchased at recess. Furthermore, Australian primary schools commonly have a fruit or vegetable break, which is a scheduled time during lessons where students eat fruit or vegetables, with >60% of NSW schools having such a fruit and vegetable break (32). Parents at these schools may be more likely to provide or purchase fruit and vegetable items for their children to consume during the fruit and vegetable break, rather than purchasing them as part of the lunchtime meal. Additionally, as fruit and vegetables are already being consumed during these breaks, parents and children may feel less inclined to purchase such products for additional eating occasions. The selection of a more popular target item may have produced different effects. Future research is required to confirm this hypothesis.

Strengths of the study included the robust study design, a large sample (i.e., 1938 students), use of objectively collected data (i.e., automatically captured by the online canteen system), and an intervention that was delivered centrally (i.e., via the online canteen ordering system), which ensured consistent implementation between schools. Furthermore, routine fidelity checks also confirmed that the intervention was maintained throughout the course of the 4-wk intervention (i.e., no new items were added that affected the implementation of the positioning strategies).

There was a relatively small number of schools in this cluster randomized controlled trial, and the included schools tended to be large with a large number of monthly orders and as such, the extent to which findings are applicable to other schools is unclear. Although schools were randomly assigned to the intervention and control arms, there were some discrepancies between groups. Control schools tended to be smaller, with fewer users of the online canteen system and fewer target items on the menus, and with users placing orders less frequently than at intervention schools. There may also have been imbalances in other characteristics that were not measured as part of the current trial, such as seasonal effects. A further limitation of the trial is that the identity of the person placing the order (i.e., parent as opposed to student) could not be ascertained with certainty. Although the online system allows separate parent and student log-ins, and the majority of orders are placed using a parent log-in, the online canteen Provider indicated that students will often order using their parents log-in, precluding any assessment of moderating effects by these variables. In addition, the listing of the target items in the first and last positions in intervention menus required the listing of each target item twice on intervention menus, compared with being listed only once on control menus. It is a limitation of the research that these effects of increased exposure cannot be isolated, and further studies should seek to address this. Furthermore, it should be noted that these trial outcomes relate to sales data. No data was collected regarding actual consumption of these items, and as such the effect of the intervention on consumption cannot be determined.

Notwithstanding these limitations, the trial findings suggest that the strategy of positioning menu items first and last on the online menu had no effect on the selection of fruit and vegetable snack items. However, given the consistent effects observed using positioning strategies in the physical food environment (i.e., placing target objects closer) additional research using a different target item (e.g., a snack or treat food) within the online environment is recommended. Further research is also warranted to better understand how consumers interact with online ordering systems, compared with traditional food ordering, with respect to habitual behaviors, and key drivers of food choice including price and availability.

As online food ordering systems become progressively more common (e.g., online grocery stores, online restaurant and fast food ordering platforms), it will be increasingly important to test whether behavioral strategies with established efficacy in the physical food environment translate to the online food environment. Use of an online canteen ordering service provides an excellent opportunity to test behavioral interventions related to the purchasing of food and drink items. Delivery of the intervention via a centrally controlled system resulted in high intervention fidelity, overcoming the issue of poor implementation that affects many trials of behavioral interventions. The increasing popularity of online canteens within primary schools enables a large volume of data to be collected in a short amount of time (i.e., over 16,000 lunch orders from 6 schools over an 8-wk trial).

Evidence from this cluster randomized controlled trial suggests that repositioning fruit and vegetable menu items to the first and last position within an online canteen menu does not increase the selection of these items for primary school students at lunchtime. Encouraging the selection of healthy foods via online environments is likely to require the use of stronger intervention strategies, more comprehensive consumer behavior interventions, and careful consideration of appropriate target menu items and purchasing contexts.

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THE EFFECT OF MEDICAL MARIJUANA LAWS ON BODY WEIGHT

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ABSTRACT

This study is the first to examine the effects of medical marijuana laws (MMLs) on body weight, physical wellness, and exercise. Using data from the 1990 to 2012 Behavioral Risk Factor Surveillance System and a difference-in-difference approach, we find that the enforcement of MMLs is associated with a 2% to 6% decline in the probability of obesity. We find some evidence of age-specific heterogeneity in mechanisms. For older individuals, MML-induced increases in physical mobility may be a relatively important channel, while for younger individuals, a reduction in consumption of alcohol, a substitute for marijuana, appears more important. These findings are consistent with the hypothesis that MMLs may be more likely to induce marijuana use for health-related reasons among older individuals, and cause substitution toward lower-calorie recreational 'highs' among younger individuals. Our estimates suggest that MMLs induce a \$58 to \$115 per-person annual reduction in obesity-related medical costs.

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KEY WORDS: medical marijuana laws; obesity; BMI

1. INTRODUCTION

As of May 2015, 23 states and the District of Columbia had enacted laws legalizing the use of marijuana for medical purposes such as treating neuropathic pain, muscle tension, anxiety, or side effects from chemotherapy.[1] While medical marijuana laws (MMLs) reduce the costs of obtaining marijuana for those suffering from legally specified health ailments, there is also evidence that the effects of MMLs may spillover into the recreational market via supply-side induced reductions in 'street prices' for marijuana (Anderson *et al.*, 2013). Proponents of MMLs often highlight their potential to generate public health benefits (Molina *et al.*, 2011; Penner *et al.*, 2013), but there is little empirical evidence on this claim. Our study is the first in the literature to estimate the impacts of MMLs on body weight, physical mobility, and diet.

The effect of MMLs on body weight is theoretically ambiguous. If MML-induced marijuana use is effective in treating physical or psychological ailments, then MMLs may increase physical activity and reduce body weight. Moreover, if marijuana and alcohol are substitutes (Anderson, Hansen, and Rees 2014; Crost and Guerrero 2012; Kelly and Rasul, 2014; DiNardo and Lemieux, 2001) and MMLs cause individuals to substitute toward marijuana and away from alcohol, a relatively high-calorie beverage, then this reduction in calories could reduce body weight. On the other hand, if marijuana use induces greater lethargy (Pesta *et al.*, 2013) or stimulates appetite (Riggs *et al.*, 2012; Soria-Gómez *et al.*, 2014), or if marijuana and alcohol are complements (Williams *et al.*, 2004; Wen *et al.*, 2014), then MMLs could increase body weight.

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[1]Four additional states (Washington, Colorado, Alaska, and Oregon) and the District of Columbia have legalized the production and consumption of marijuana more broadly, including for recreational use (ProCon.org, 2015)

A number of empirical studies have examined the relationship between marijuana use and body weight, but almost all have treated marijuana use as exogenous to body weight (for example, Le Strat and Le Foll, 2011; Rodondi *et al*., 2006). This assumption is problematic if difficult-to-measure characteristics—such as personality and personal discount rates—are related to both marijuana consumption and body weight. Moreover, if individuals use marijuana to treat health problems related to obesity (Pagoto *et al*., 2012), reverse causality could also lead to biased estimates.

Our study is the first to examine the effect of MMLs—a plausibly exogenous policy change that has been documented to increase marijuana use among adults (Anderson and Rees 2011; Wen *et al*. 2014)—on body weight, physical well-being, and physical activity. Using data from the 1990 to 2012 Behavioral Risk Factor Surveillance System (BRFSS) and a difference-in-differences approach, we find that the enforcement of MMLs is associated with a 0.4% to 0.7% reduction in body mass index (BMI) and a 2% to 6% reduction in obesity. The estimated magnitudes of these effects are 1.6 to 3.2 times larger in the longer run, consistent with the hypothesis that body weight effects occur with a lag. Our findings are robust to falsification tests on policy leads, the inclusion of controls for state-specific time trends, and the use of a synthetic control state for each state that enacted an MML.

We find some evidence that mechanisms driving the reduction in body weight differ across the age distribution. The MML-induced reduction in BMI we observe for those ages 18–24 years appears largely driven by a reduction in the consumption of alcohol, a relatively high-calorie beverage (Nielsen *et al*., 2012). This suggests that MMLs are reducing younger individuals' body weight via substitution of recreational substances toward a less caloric recreational high. For older individuals, MMLs also appear to increase physical mobility and wellness, suggesting that MML-induced marijuana use may also occur for pain-reducing purposes.

2. BACKGROUND

The legalization of medical marijuana has been the subject of intense political debate for the last two decades. Advocates for MMLs emphasize the effectiveness of marijuana in treating symptoms of illnesses associated with neuropathic pain, muscle tension, anorexia, arthritis, cancer, wasting syndrome, Crohn's disease, diabetes, HIV/AIDS, multiple sclerosis, and Parkinson's disease (Galuppo *et al*., 2014; Lotan *et al*., 2014; Naftali *et al*., 2013; Vu *et al*., 2014).[2] They also cite the potential of MMLs to reduce alcohol-related traffic fatalities (Anderson *et al.*, 2013) and suicides (Anderson *et al*. 2014).

However, opponents of legalization argue that there may be negative health consequences of MMLs, particularly because of their spillovers into the recreational market. Both Anderson and Rees (2011) and Wen *et al*. (2014) find evidence that MMLs increase marijuana use among young adults, who have relatively low probabilities of suffering from medical ailments. In addition, Wen *et al.* (2014) also find that MMLs increase marijuana use on the extensive margin, increasing the share of daily users of marijuana. Opponents argue that MML-induced recreational marijuana use may serve as a 'gateway' to greater addiction (Hall, 2009a; Miron, 2005). According to the 2011 National Survey on Drug Use and Health, an estimated 4.3 million people struggle with marijuana dependence, a number which exceeds the current number of dependent abusers of pain relievers, cocaine, and heroin combined (Substance Abuse and Mental Health Services Administration, 2013). In addition, skeptics of legalization argue that marijuana use could lead to impaired respiratory function, cardiovascular disease, psychotic symptoms (Hall & Degenhardt, 2009b), and even more crime (Pederson & Skardhamar, 2010).

Could increased marijuana use induced by MMLs affect body weight? If so, these health effects could be important. More than two-thirds of US adults (68.8%) are overweight or obese (Flegal *et al*., 2012). Obesity is associated with a \$2741 per person increase in annual medical expenses (Cawley and Meyerhoefer, 2012)

[2]Advocates of legalization also cite potential tax revenues from legalized marijuana appeal to state governments struggling to meet debt obligations (Caputo & Ostrom, 1994; Miron, 2005).

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and has been linked to poorer labor market outcomes (Cawley 2004; Greve, 2008; Tunceli *et al*., 2006), diminished academic performance (Sabia 2007) and reduced educational attainment (Rees and Sabia 2015). Obesity is also linked to a number of health ailments that impede mobility (Vincent *et al*. 2010), including increased physical pain (Heim *et al*., 2008), reduced upper and lower body function (Apovian *et al*., 2002), and increased risk of joint replacement surgery (Harms *et al*., 2007). In the succeeding sections, we discuss a number of theoretical mechanisms through which MMLs could affect body weight.

2.1. Physical health

Pain is debilitating (Heim *et al*., 2008). Medical marijuana is often prescribed to treat chronic neuropathic pain, muscle tension, and arthritis (Galuppo *et al*., 2014; Lotan *et al*., 2014; Naftali *et al*., 2013; Vu *et al*., 2014). There is medical evidence suggesting that marijuana is effective in alleviating pain associated with these ailments (Rog *et al*. 2005, Ware *et al*. 2010). For example, in a randomized control trial, cannabis-based medicine was found to produce a 'significant analgesic effect' for patients suffering from rheumatoid arthritis (Blake *et al*., 2006). Furthermore, marijuana has been shown to relieve pain for patients suffering from fibromyalgia (Fiz *et al*., 2011). Other studies have established the effectiveness in ameliorating the side effects from aggressive treatments for cancer (Hall *et al*., 2005; Doblin and Kleinman, 1991; Vinciguerra *et al*., 1988). If MML-induced marijuana use is effective in alleviating physical pain—particularly pain associated with mobility—then MMLs could decrease obesity by increasing the likelihood and frequency of engaging in regular physical activity. In addition, MMLs may induce individuals suffering from health ailments to substitute marijuana for prescription and over-the-counter pharmaceutical drugs, perhaps both legally and illicitly. To the extent that some pharmaceutical drugs possess obesogenic side effects such as bloating, slowing of digestion and metabolism, and weight gain (Domecq *et al*., 2015; Hasnain *et al*., 2012) that are not present (or are not as severe) in marijuana, MMLs may result in lower body weight through such substitution effects.

Absent chronic pain symptoms, the effect of marijuana use on physical activity is less clear. There is evidence that repeated marijuana use slows the body's resting heart rate (Jones, 2002), reduces athletic performance (Pesta *et al*., 2013), and induces lethargy (Delisle *et al*., 2010; Irons *et al*., 2014; Pate *et al*., 1996). These effects could reduce expenditure of calories and increase the likelihood of obesity.

2.2. Appetite and diet

There is a growing medical evidence to suggest the existence of a number of neurophysiological pathways through which cannabis consumption affects appetite (Soria-Gómez *et al*., 2014). Tetrahydrocannabinol (THC) is the active ingredient in marijuana that is responsible for its psychoactive effects and most likely responsible for its effects on appetite. THC is one of the many cannabinoid-like molecules known as exogenous cannabinoids because they enter the body via external means (i.e., the consumption of marijuana). Endocannabinoids are cannabinoid molecules that exist naturally in the body. They link up to a receptor, called the CB receptor, which influences areas of the body related to appetite including the gastrointestinal system, which moderates food intake; the hypothalamus and hind brain, which regulate food intake; stomach and intestinal tissue, which send signals of satiation to the brain; and the limbic forebrain, which affects the palatability of food. Exogenous cannabinoids work within the body the same way endocannabinoids function by mimicking them and binding to CB receptors (De Fonseca *et al*., 2005). As Kirkham (2005) notes

> *It is increasingly apparent that the changes in eating motivation associated with cannabis intoxication, or the administration of THC, reflect a crucial role for these endocannabinoid systems in the normal processes governing appetite, ingestive behavior, energy metabolism, and body weight. (pp. 297)*

Early randomized control trials provide evidence that marijuana use leads to increased appetite and caloric intake (Greenberg *et al*. 1976; Foltin *et al*. 1988; Mattes *et al*., 1994; Berry and Mechoulam, 2002). Consistent with the popular notion of the 'munchies', an experimental study by Foltin *et al*. (1988) finds that increased

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caloric consumption subsequent to active THC consumption was primarily driven by more between-meal snacking, particularly of sweet solid items like candy bars.

Although marijuana and its pharmacological derivatives were initially prescribed to cancer patients to alleviate symptoms of nausea and vomiting, clinical trials soon revealed that it was effective in stimulating the appetite of those undergoing chemotherapy (Gorter, 2004). It has since been used to combat the wasting syndrome that accompanies aggressive medical treatment of cancers and HIV (Musty and Rossi, 2001; Ungerleider and Andrysiak, 1982). Science continues to illuminate the link between the endocannabinoid system and how we experience food. Most recently, Soria-Gómez *et al.* (2014) found that the endocannabinoid and olfactory systems are connected, which could explain why users of marijuana report heighted sense of smell and taste.

A second pathway through which MMLs could affect diet (and therefore body weight) is through their effect on the relative prices of other substances. There is substantial evidence that consumption of marijuana and alcohol are related. Anderson and Rees (2013) find evidence that MMLs are associated with a reduction in alcohol consumption, beer sales, and alcohol-related traffic fatalities, suggesting that marijuana and alcohol are substitutes.[3] A number of other studies come to a similar conclusion using plausibly exogenous variation in the minimum legal drinking age (Crost and Guerrero, 2012; Crost and Rees, 2013).

How large might we expect body weight reductions to be from MML-induced declines in alcohol consumption? The average serving of beer consists of roughly 150 cal, while the average serving of wine and spirits is approximately 120 cal (Nielsen *et al*., 2012). Estimates from Anderson and Rees (2013) and Anderson *et al.* (2014) suggest that MMLs are associated with a 10.6% to 25.2% reduction in the number of drinks consumed per month among 20- to 29-year olds. This would imply, ceteris paribus, an MML-induced reduction of 360 to 570 cal per month, or approximately 2 lbs of body weight for each year an MML is enforced. These effects may be larger if MMLs reduce heavy episodic drinking, including binge drinking, which involves consuming five or more drinks on a single occasion.

Estimated body weight effects may be more pronounced if MMLs affect *where* people are consuming calories. For example, if MMLs induce more *in-home* marijuana consumption and less *at-bar* or *in-restaurant* alcohol consumption, meals and snacks that are often paired with alcohol at bars and restaurants may also be consumed less frequently. If these foods contain more calories than in-home foods, such food substitution could lead to decreased body weight (for example, McCrory *et al*., 1999; Currie *et al*., 2010; Davis and Carpenter, 2009).[4]

In addition to alcohol, consumption of other substances—including cigarettes or illicit drugs—may be affected by MMLs, which, in turn, may affect body weight. Some recent evidence from Choi *et al*. (2015) suggests that MMLs could affect the demand for cigarettes. Because tobacco is known to be an appetite suppressant (Chen *et al*., 2005), MML-induced cigarette consumption could be a pathway through which MMLs affect obesity. Moreover, while there is scant evidence that MML-induced marijuana use acts as a gateway to harder drugs (Wen *et al*. 2014), if MMLs affect consumption of harder drugs such as cocaine or methamphetamine, known appetite suppressants, this could lead to a reduction in body weight.

A final diet-related path through which MMLs could affect body weight is if marijuana consumption takes the form of marijuana-infused ‘edibles’, which could contribute directly to energy intake (Kuddus *et al*., 2013). However, given that even very small amounts of edibles contain potent doses of marijuana with long-lasting psychoactive effects, and overconsumption typically leads to unpleasant side effects (Murphy *et al*., 2015; Armentano, 2005), this channel is likely to be a relatively less important driver of changes in body weight.

[3]Depenalization of marijuana in the UK has also been found to be associated with a decrease in alcohol-related hospital admissions (Kelly and Rasul, 2014).

[4]Existing empirical evidence on the relationship between restaurant food consumption and obesity seems at least somewhat dependent on identification strategy employed. For example, Currie *et al*. (2009) and Davis and Carpenter (2009) find that close proximity to nearby restaurants is associated with increased risk of obesity. On the other hand, Anderson and Matsa (2011) find that increased restaurant food consumption induced by close proximity to restaurants (using plausibly exogenous variation in the historical placement of Interstate highways as an instrument) does not lead to increased obesity.

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2.3. Mental health and healthcare services

Another mechanism through which marijuana use could affect body weight is through its effects on psychological well-being. Cannabinoids have been shown to produce antidepressant-like behavior in animals, suggesting that marijuana use may improve mental and emotional well-being (Bambico *et al*., 2007). While a number of correlational studies have found that marijuana use and depression are positively related (Degenhardt *et al*., 2003; Green and Ritter, 2000), a recent study by Anderson *et al*. (2014) find that this can largely be explained by the endogeneity of marijuana use. In fact, they find that the enactment of MMLs is associated with a nearly 5% reduction in suicides, suggesting potentially important mental health benefits of MMLs. Improved mental health has been found to be associated with greater exercise (Paluska and Schwenk, 2000; Stephens 1988) and better dietary habits (Oddy *et al*., 2009), and could be associated with greater efficiency in the choice of inputs to produce physical health.

Finally, MMLs may lead to greater utilization of healthcare services, which could affect body weight. Specifically, individuals who seek a prescription for medical marijuana may have increased contact with their healthcare providers. Increased contact with healthcare providers could lead to improvements in patients' behavioral and mental health via counseling, information, and provision of medical advice.

2.4. Studies on the effect of marijuana on body weight

A number of studies have examined the relationship between marijuana use and obesity, with the majority finding that marijuana use is negatively correlated with body weight (Le Strat and Le Foll, 2011; Rodondi *et al*., 2006). However, most of these studies assume that marijuana use is econometrically exogenous, which may be problematic given the possibility of both reverse causality and individual unmeasured heterogeneity leading to biased estimates. French and Norton (2010), for example, highlight the empirical challenge in establishing a causal link between substance use and body weight:

> *Estimation of single-equation models will generate consistent coefficient estimates only if no unobservable omitted variables are correlated with [consumption]. Two examples of potentially important omitted variables are dieting practices and chronic eating disorders…Without better measures of eating behaviors and other personality traits, the [substance use] variables in a single-equation model could be picking up the effects of other behaviors and traits, thereby introducing bias into the coefficient estimates. The direction of the omitted variable bias is theoretically indeterminate because it depends not only on the nature of the omitted variables but also on the correlations among the covariates. (p. 5)*

Beulaygue and French (2014) use longitudinal data from the National Longitudinal Study of Adolescent Health and find that the negative relationship between marijuana use and body weight is robust to time-invariant individual unobserved variables. But the use of individual fixed effects models cannot rule out the possibility that these findings are explained, in part or in whole, by reverse causality or time-variant unobserved variables.[5]

We contribute to the above literature by examining the effect of MMLs on body weight. In addition, this study is the first to explore the effect of MMLs on physical well-being, mobility, and diet, all important mechanisms through which marijuana use may affect body weight.

[5]Foltin *et al*. (1988) attempt to overcome the endogeneity of marijuana consumption using a randomized control trial. However, the study lacks generalizability as it only consists of six volunteer subjects.

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3. DATA

Our analysis uses data from the BRFSS, a repeated cross-sectional nationally representative survey conducted annually by the Centers for Disease Control and Prevention (CDC) since 1984. While the BRFSS was administered via landline telephone through 2010, beginning with the 2011 survey, the CDC began adding cellular phones to the BRFSS sample and weighted these respondents accordingly. Respondents, aged 18 to 99 years, are asked detailed questions about their health and health behavior. Our primary analysis sample consists of approximately 5.4 million observations drawn from the years 1990 through 2012.

3.1. Body weight

Our primary outcome variable of interest, *BMI*, is measured in the BRFSS core survey and is calculated using the respondent's self-reported weight in pounds divided by his or her height in inches squared, multiplied by 703. From this continuous BMI measure, we also generate an indicator of obesity status, *Obese*, which is set equal to 1 if the respondent reports a BMI score of 30 or above and 0 otherwise, following the CDC obesity classification (Center for Disease Control and Prevention, 2014).

Table I presents unweighted means for *BMI* and *Obese*, first for the full sample, and then by age. For the full sample, the mean BMI is 27.0, which is considered overweight according to the Center for Disease Control and Prevention (2014). BMI increases with age, rising from an average of 24.6 for 18- to 24-year-olds to 27.4 for individuals 50 years and older. In our sample, 24% of all individuals were obese, with the percentages also rising with age.

3.2. Mechanisms

In addition to body weight, the BRFSS asks respondents a number of questions we use to measure mechanisms through which MMLs may affect body weight. First, as part of the 1993–2012 BRFSS core questionnaires, respondents are asked about the state of their physical health:

> *Now thinking about your physical health, which includes physical illness and injury, for how many days during the past 30 days was your physical health not good?*

We generate a dichotomous variable equal to zero if the respondent reported zero poor physical health days in the last month, and equal to one if the respondent has reported a positive number of poor physical health days in the last month. For the full sample, 35.7% of respondents reported having at least one poor health day in the last 30 days.

Respondents are also asked about the frequency with which they engage in physical activity. In odd years between 2003 and 2011 (and as a module question in 2002), respondents are asked about time spent exercising:

> *Now thinking about the vigorous physical activities you do [fill in "when you are not working" if 'employed' or 'self-employed' in a usual week, [how often] do you do vigorous activities for at least 10 minutes at a time, such as running, aerobics, heavy yard work, or anything else that causes large increases in breathing or heart rate?*

For the full sample, about 40.3% of respondents reported engaging in any minimal exercise, defined as at least 30 min of exercise during an average week over the last month. Conditional on any minimal exercise, the average weekly time spent exercising was 226.1 min per week. Finally, we examine more frequent exercise to further capture the intensive margin. Conditional on having performed any minimal exercise in the last week, we generate a dichotomous variable equal to one if the respondent performed at least 90 min of exercise on average per week, and zero if they performed less than 90 min per week. Approximately 70% of those who reported minimal exercise performed at least 90 min per week.

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Table I. Means of Key Variables

Variable	Mean	Standard Deviation	N
BMI			
Full Sample	27.01	5.73	5428399
18-24	24.64	5.16	316544
25-34	26.29	5.72	722704
35-49	27.22	5.93	1445107
50+	27.37	5.61	2915976
Obese			
Full Sample	0.240	0.4272	5428399
18-24	0.131	0.3378	316544
25-34	0.204	0.4032	722704
35-49	0.250	0.4332	1445107
50+	0.257	0.4369	2915976
Any Poor Physical Health Days in Last 30 Days			
Full Sample	0.357	0.479	5176842
18-24	0.357	0.479	287599
25-34	0.332	0.471	669034
35-49	0.346	0.476	1377555
50+	0.369	0.482	2802428
Drink Any Alcohol in Last 30 Days			
Full Sample	0.501	0.500	5181180
18-24	0.5525	0.4972	286441
25-34	0.59	0.4918	667771
35-49	0.5737	0.4945	1359434
50+	0.4411	0.4965	2828214
Binge Drank in Last 30 Days			
Full Sample	0.1199	0.3249	5127666
18-24	0.2705	0.4442	283118
25-34	0.2169	0.4121	661835
35-49	0.1576	0.3644	1346269
50+	0.0645	0.2456	2798323
Any Minimal Exercise in Last Month (*Extensive Margin*)			
Full Sample	0.403	0.491	1650516
18-24	0.596	0.491	83103
25-34	0.540	0.498	203563
35-49	0.490	0.500	448655
50+	0.312	0.463	902953
Minutes of Exercise in Last Month (*Intensive Margin*)			
Full Sample	226.13	307.62	665592
18-24	265.45	337.40	49504
25-34	214.03	289.30	109943
35-49	213.38	293.66	220050
50+	233.92	318.72	281655
Any Exercise Over 90 Minutes (*Intensive Margin*)			
Full Sample	0.728	0.445	665592
18-24	0.763	0.425	49504
25-34	0.727	0.446	109943
35-49	0.719	0.449	220050
50+	0.729	0.444	281655
Food Consumption			
Full Sample	118.42	67.75	2262538
18-24	108.33	72.79	140954
25-34	110.97	67.37	332665
35-49	113.89	66.56	657606
50+	124.44	67.24	1117259

(*Continues*)

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Table I. (Continued)

Variable	Mean	Standard Deviation	N
Poor Mental Health in Last 30 Days			
Full Sample	0.318	0.466	5191627
18-24	0.462	0.499	287738
25-34	0.403	0.491	667574
35-49	0.379	0.485	1374420
50+	0.255	0.436	2821617
Independent Variables			
MML	0.185	0.385	5428399
Male	0.406	0.491	5428399
Married	0.549	0.498	5414325
Age	51.57	17.81	5426971
White	0.851	0.356	5383327
Black	0.084	0.277	5383327
Hispanic	0.058	0.234	5404882
Some high school	0.069	0.254	5419941
High school graduate	0.309	0.462	5419941
Some College	0.268	0.442	5419941
College +	0.318	0.466	5419941
Unemployment Rate	6.02	2.18	5428399
Prime-Age Male Average Wage Rate	15.64	3.30	5428399

Note: For some independent variables, missing observations are included in each regression, which leads to some differences in the number of actual observations

A third mechanism we measure in the BRFSS is alcohol consumption. Alcohol consumption is measured in two ways. First, we measure whether an individual reported drinking any alcoholic beverages such as beer, wine, malt beverages, or liquor in the prior 30 days. Data on alcohol consumption are available as a core question throughout the entire sample except for even years from 1994 to 2000 when alcohol consumption is a module question. We first generate an alcohol consumption measure equal to 1 if a respondent reported any alcohol consumption in the last 30 days and 0 otherwise. We find that 50.1% of all respondents reported consumption of at least one drink of alcohol in the last 30 days. Second, we measure binge drinking using responses to the following questionnaire item:

> *Considering all types of alcoholic beverages, how many times during the past 30 days did you have 5 or more drinks on an occasion?*

If a respondent reported being a non-drinker or answered the aforementioned question in the negative, our binge drinking measure is set equal to zero; if the respondent reported binge drinking at least once in the last 30 days, our binge drinking measure is set equal to zero. Approximately 12% of the full sample binge drank at least once in the last 30 days.[6]

The BRFSS also asks questions about food consumption and diet consistently as part of their core questionnaire between 1990 and 1992, and then odd years from 1993 to 2001. Module questions are asked in even years from 1994 to 2002, and then odd years from 2003 to 2009. However, the BRFSS only contains information on *healthy* food consumption during the years when states changed MMLs.[7] To estimate total monthly food consumption, we sum how often respondents eat potatoes, fruit juice, green salad, carrots, and vegetables. The average number of times individuals have eaten any of these foods is about 118 times in the last month.

Finally, respondents are asked about their psychological well-being as part of the core questionnaire from 1993 to 2012 except for 2002 where it is part of the module questionnaire. Respondents are asked

[6]In addition to these drinking measures, we also experimented with measures of number of drinks in the last month. The results are generally qualitatively similar to those presented using our dichotomous measures.

[7]The BRFSS asks about unhealthy food consumption during the years 1990–1994, but no states changed MMLs during this time period.

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Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?

We generate a mental health measure set equal to zero if the respondent reported zero days of poor mental health, and equal to one if the respondent reported positive days of poor mental health. For the full sample, about 32% of respondents reported at least one poor mental health day in the last 30 days.

3.3. Strengths and weaknesses of the BRFSS

We use the BRFSS for our analysis because it provides consistent, high-quality nationally representative data on body weight and the mechanisms that affect body weight during the sample period over which states enacted MMLs. But there are a number of limitations worthy of note. Both our BMI and the mechanisms described previously are self-reported, likely introducing measurement error. Alternative measures, such as percent body fat (Burkhauser and Cawley, 2008) and skinfold thickness (Burkhauser, Cawley, and Schmeiser, 2009) might also better capture healthy and unhealthy body weight. But this measurement error in our left-hand side variable should not generate biased estimates of the impact of MMLs on these outcomes unless this measurement error is correlated with the enforcement of MMLs.

A second limitation with the BRFSS is that marijuana use is not measured in these data. Therefore, we cannot obtain 'first-stage' estimates of the effect of MMLs on marijuana consumption so as to evaluate the effect of MMLs on the body weight of those who consume marijuana. Two studies, however, have found that MMLs are associated with an increase in marijuana use among adults. Anderson and Rees (2011) and Wen *et al.* (2014) use data from the National Survey of Drug Use and Health (NSDUH) and find that MMLs increase marijuana use among adults, on average, by 16% to 19%.[8] However, neither the NSDUH nor the BRFSS allow researchers to distinguish between medical users and recreational users; thus, estimated effects of MMLs—in their studies and ours—capture total effects on registered medicinal users, unregistered self-medicating patients, and recreational users.

As of October 2014 there were 1,137,069 registered patients in 19 out of 23 states that have MMLs and report numbers of patients (ProCon.org, 2015). This comprises approximately 1.0% of the adult populations of U.S. Census Bureau (2014) and 15.3% of all marijuana users (SAMHSA, 2013) in these states.[9] Thus, the magnitude of the effects observed by Anderson and Rees (2011) and Wen *et al.* (2014)—as well as our own estimates—are likely not driven entirely by medical marijuana users.

A third limitation of the BRFSS is that it does not provide a measure of total caloric intake nor of consumption of unhealthy foods during the period MMLs were enacted. Therefore, our analysis of the effect of MMLs on diet will be limited and may capture inter-diet substitution effects toward or away from healthy foods. The BRFSS also lacks information on where food consumption takes place (e.g., at home or at bars or restaurants), which do not allow us to explore the extent to which MMLs might affect meal and snack consumption at restaurants or bars.

A final limitation concerns our mechanism measures as a whole. Because these variables are measured concurrently with body weight (rather than prior to body weight changes), it is possible that they may capture consequences rather than causes of MML-induced changes in body weight. For instance, MML-induced improvements in physical mobility may be a result rather than a cause of reduced body weight. Therefore, we are careful in our interpretation of findings on these mechanisms.

3.4. Medical marijuana law

Our key independent variable of interest is an indicator for the share of the year that a state had enacted a medical marijuana law. We follow the coding of effective dates provided by Anderson and Rees (2013)

[8]Anderson, Hansen, and Rees (forthcoming) finds no evidence that marijuana use affects minor teenage marijuana consumption using data from the Youth Risk Behavior Survey.
[9]The latter estimate is obtained using prior 30-day marijuana use as reported in the most recent wave of the National Survey of Drug Use and Health (SAMHSA, 2013).

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Table II. States enacting and enforcing medical marijuana laws, 1990–2013

State	Effective date
Alaska	March 4, 1999
Arizona	April 14, 2011
California	November 6, 1996
Colorado	June 1, 2001
Connecticut	October 1, 2012
Delaware	May 13, 2011
District of Columbia	June 27, 2010
Hawaii	December 28, 2000
Illinois	January 1, 2014
Maine	December 22, 1999
Massachusetts	January 1, 2013
Michigan	December 4, 2008
Montana	November 2, 2004
Nevada	October 1, 2001
New Hampshire	July 23, 2013
New Jersey	October 1, 2010
New Mexico	July 1, 2007
Oregon	December 3, 1998
Rhode Island	January 3, 2006
Vermont	July 1, 2004
Washington	November 3, 1998

These dates are effective dates for state-level medical marijuana laws and are gathered from the National Conference of State Legislatures (2014), Anderson *et al.* (2013), and Eddy (2010).

and updated by Wen *et al.* (2014), which we show in Table II. There is a broad agreement in the literature that MMLs are not homogeneous across states (Anderson and Rees, 2013; Anderson and Rees 2014; Pacula *et al*., 2015). Therefore, we also explore whether our results are driven by particular types of MML, such as those that allow for home or collective cultivation of marijuana or do not strictly regulate dispensaries.

3.5. Event studies

In Figures 1 and 2, we present trends in BMI and obesity, respectively, for states that implemented MMLs and states that did not implement MMLs during the 1990 to 2012 period. The marker for 'year zero' indicates the year of passage of an MML in a treated state, and a randomly assigned treatment date for the control states. Prior to the enactment of MMLs, the trend in body weight in treatment and control states was relatively similar. However, after year 0, the rate of increase in body weight in the treatment states slows relative to the control states. This event study provides some descriptive evidence that MMLs are associated with declines in body weight. We explore this possibility further with difference-in-difference and synthetic control models.

4. METHODS

Following Anderson *et al.* (2013), we begin with a difference-in-difference model of the following form:

$$Y_{\mathrm{ist}} = \beta_0 + \beta_1 \mathrm{MML}_{\mathrm{st}} + X_{\mathrm{st}}\beta_2 + Z_{\mathrm{ist}}\beta_3 + v_{\mathrm{s}} + \omega_{\mathrm{t}} + \varepsilon_{\mathrm{ist}} \tag{1}$$

where Y_{ist} measures body weight of individual i residing in state s in year t, MML is an indicator for whether state s is enforcing a medical marijuana law in year t, X_{st} is a vector of state-level time-varying controls, Z_{ist} is a vector of individual-level time-varying controls. The remaining terms, v_s and ω_{t}, represent state fixed effects and year fixed effects, respectively. Included in the vector X_{st} are the state unemployment rate, prime-age (ages

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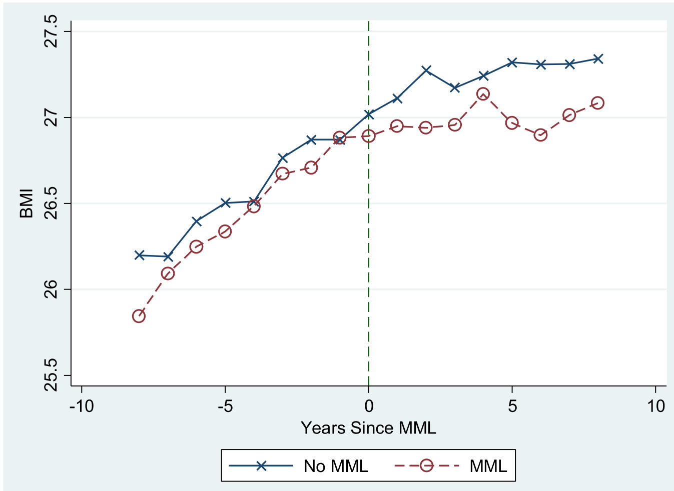

Figure 1. Event study of BMI before and after MML implementation

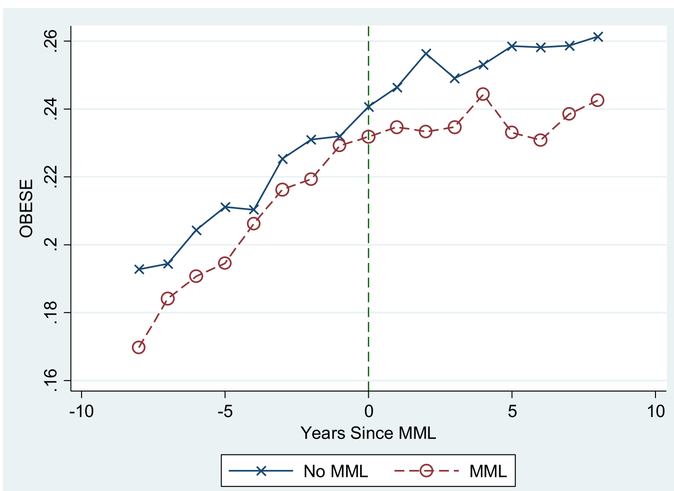

Figure 2. Event study of obesity before and after MML implementation

25 to 54 years) average wage rate, beer taxes, cigarette taxes, marijuana decriminalization law, and food prices.[10] Included in the vector Z_{ist} are age, gender, race/ethnicity, marital status, and indicators for educational attainment. We also experiment with up to 5-year lags of the medical marijuana measure given that the impact on body weight may take time to materialize.

The key coefficient of interest, β_1, is the estimated relationship between medical marijuana laws and body weight. Identifying variation in Equation (1) comes from the 17 states and the District of Columbia

[10]Unemployment rate data are collected at the state level from the Bureau of Labor Statistics Local Area Unemployment Statistics, average state level wages from the Current Population Survey for 1990–2012, beer taxes from the Brewers Almanac 2012 (Beer Institute, 2012), cigarette taxes from the *Tax Burden on Tobacco* (Tobacco Institute, 2014), and food prices from The Council for Community and Economic Research. Food prices included are based on coding conventions used in Beydoun *et al*. (2008) and consist of potatoes, bananas, lettuce, sweet peas, tomatoes, peaches, frozen corn, hamburgers, pizza, and fried chicken.

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that changed their MMLs during the 1990 to 2012 period. In all models, standard errors are clustered by state.

Credible identification of β_1 relies on the assumption that body weight trends in states that did not implement MMLs are a reasonable counterfactual for trends in MML states had they not implemented MMLs. Inasmuch as differential trends and policy endogeneity remain a concern, we take a number of tacks to address this issue. First, we add state-specific quadratic time trends as additional controls in Equation (1).[11] Second, we test whether there are differential body weight trends prior to the implementation of an MML by including leads of the MML on the right-hand side of Equation (1).

Finally, we pursue a synthetic control design approach following Abadie *et al*. (2010) to ensure that pre-treatment health trends are common between treatment and control states. The counterfactual synthetic control for each treatment state (defined as a state that implemented an MML between 1990 and 2012) is generated as a linear combination of donor states, where donor states include all states that do not have MMLs enforced at any time between 1990 and 2012. The weight that each donor state contributes to the synthetic control state is determined by an algorithm pioneered by Abadie *et al*. (2010) that assigns synthetic weights to each donor state to minimize pre-treatment differences in body weight and state-level covariates between each treatment and synthetic state. Each treatment state and its synthetic control state are then pooled, and Equation (1) is re-estimated, with clustered bootstrapped standard errors.

There are a few important advantages to a synthetic approach. Forcing counterfactuals to have more similar pre-treatment trends may increase the probability of satisfying the common trends assumption (Sabia *et al*., 2012; forthcoming). Moreover, because we construct a counterfactual to each MML state, this approach more flexibly allows for heterogeneity in the impacts of MMLs across different states.

5. RESULTS

5.1. Difference-in-differences estimates

Table III presents difference-in-differences estimates of the effect of MMLs on BMI.[12] Panel I shows the contemporaneous effects. Controlling for only economic and demographic characteristics (column 1), we find that MMLs are associated with a 0.6% (0.162/27.00) decline in BMI.[13] After adding controls for alcohol policies, cigarette taxes, and marijuana decriminalization laws (column 2), and food prices (column 3), the magnitude of the association falls only slightly.[14] Finally, when we control for state-specific quadratic time trends (column 4), we find MMLs are associated with a 0.31% (0.084/27.00) reduction in BMI. Because changes in BMI in response to MMLs may take time to occur, we next include 5 years of lags of MMLs (panel II). Our results suggest that the impact of MMLs on BMI is substantially larger in the longer run as compared with the short run, as estimated effects become larger (in absolute magnitude) the longer the MML is enforced.[15]

[11]We also experimented with linear time trends and state-specific trends of higher-order polynomial (e.g., cubic trends) and find that difference-in-difference estimates from Equation (1) as well as models that include quadratic or cubic time trends all show a similar pattern of results: a negative effect of MMLs on body weight. Models with linear time trends show smaller effects. The results from each of these specifications are available upon request.

[12]The estimates presented in our main tables are unweighted. Weighted difference-in-difference estimates of MMLs on BMI and obesity are available upon request of the authors and suggest a similar pattern of results.

[13]We estimate the percent change by dividing the estimated marginal effect of the MML by the mean of the outcome for states and years in which there is no MML, following Anderson and Rees (2013).

[14]The results are also robust to controls for state-specific time-varying anti-marijuana legalization sentiment, which was generated using data from the General Social Survey. This suggests that our estimate of β_1 is not driven by within-state over time changes in attitudes toward marijuana legalization.

[15]If we exclude individuals from the sample who were younger than 21 years or younger than 23 years—to ensure that those who lived in an MML state were affected for at least 3 or 5 years, respectively, after the age of 18 years—results are quantitatively and qualitatively similar. These results are available upon request of the authors.

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Table III. Difference-in-difference estimates of the relationship between MMLs and BMI

	(1)	(2)	(3)	(4)
Panel I: contemporaneous effects				
MML	−0.162*** (0.046)	−0.134*** (0.044)	−0.112*** (0.038)	−0.084** (0.034)
Panel II: lagged effects				
Year of law change	−0.117*** (0.036)	−0.100*** (0.035)	−0.105*** (0.037)	−0.088** (0.033)
1 year after MML	−0.078** (0.036)	−0.069** (0.032)	−0.067* (0.037)	−0.058 (0.043)
2 years after MML	−0.160*** (0.050)	−0.148*** (0.047)	−0.164*** (0.046)	−0.159*** (0.050)
3 years after MML	−0.159*** (0.053)	−0.137** (0.052)	−0.132** (0.051)	−0.134** (0.058)
4 years after MML	−0.060 (0.063)	−0.034 (0.059)	−0.014 (0.052)	−0.023 (0.057)
5+ years after MML	−0.243*** (0.069)	−0.203*** (0.070)	−0.157** (0.066)	−0.116* (0.061)
Mean BMI (MML = 0)	27.000	27.000	27.000	27.000
Demographic and economic controls	Yes	Yes	Yes	Yes
State policy controls	No	Yes	Yes	Yes
Food prices	No	No	Yes	Yes
State time trends	No	No	No	Yes
N	5,428,399	5,428,399	5,428,399	5,428,399

Note: Each column represents a result from separate unweighted regressions that include state and year fixed effects. Demographic and economic controls include gender, race (White, Black, and Hispanic), education, marital status, average wage by state and year (Current Population Survey), and state-level unemployment rate (Bureau of Labor Statistics Local Area Unemployment Statistics). State-level policy controls include marijuana decriminalization laws, zero-tolerance laws, and state-level alcohol and cigarette taxes. Food prices are collected from American Chamber of Commerce Research Association survey (ACCRA) and include prices for potatoes, bananas, lettuce, sweet peas, tomatoes, peaches, frozen corn, hamburger, pizza, and fried chicken. Standard errors are below each coefficient estimate in parentheses and are clustered by state. State time trends consist of interacting a linear time and a squared time variable with state fixed effects to generate a state-specific quadratic time trend.
*Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

Table IV. Difference-in-difference estimates of the relationship between MMLs and obesity

	(1)	(2)	(3)	(4)
Panel I: contemporaneous effects				
MML	−0.010*** (0.003)	−0.009*** (0.003)	−0.007** (0.003)	−0.005* (0.003)
Panel II: lagged effects				
Year of law change	−0.005 (0.003)	−0.004 (0.003)	−0.005 (0.003)	−0.003 (0.003)
1 year after MML	−0.006** (0.003)	−0.005** (0.002)	−0.005* (0.003)	−0.004 (0.003)
2 years after MML	−0.010** (0.004)	−0.009** (0.004)	−0.010*** (0.003)	−0.008** (0.003)
3 years after MML	−0.011*** (0.004)	−0.010** (0.004)	−0.009** (0.004)	−0.008* (0.005)
4 years after MML	−0.005 (0.004)	−0.004 (0.004)	−0.002 (0.004)	−0.001 (0.005)
5+ years after MML	−0.015*** (0.004)	−0.013*** (0.005)	−0.009** (0.004)	−0.005 (0.005)
Mean obesity (MML = 0)	0.240	0.240	0.240	0.240
Demographic and economic controls	Yes	Yes	Yes	Yes
State policy controls	No	Yes	Yes	Yes
Food prices	No	No	Yes	Yes
State time trends	No	No	No	Yes
N	5,428,399	5,428,399	5,428,399	5,428,399

Note: Each column represents a result from separate unweighted regressions that include state and year fixed effects. Demographic and economic controls include gender, race (White, Black, and Hispanic), education, marital status, average wage by state and year (CPS), and state-level unemployment rate (BLS LAUS). State-level policy controls include marijuana decriminalization laws, zero-tolerance laws, and state-level alcohol and cigarette taxes. Food prices are collected from ACCRA and include prices for potatoes, bananas, lettuce, sweet peas, tomatoes, peaches, frozen corn, hamburger, pizza, and fried chicken. Standard errors are below each coefficient estimate in parentheses and are clustered by state. State time trends consist of interacting a linear time and a squared time variable with state fixed effects to generate a state-specific quadratic time trend.
*Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

In Table IV, we replace our continuous measure of BMI with a dichotomous indicator of obesity. The pattern of results is consistent with what we find in Table III. Controlling for all observables and state-specific time trends (panel I, column 4), we find that the enforcement of MMLs is associated with a 2.1% (0.005/0.240)

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reduction in obesity. Across specifications, we generally find the largest effects on obesity 5 or more years after implementation with estimated effects reaching as large as 6.2% (0.015/0.240) (panel II, column 1).

One possible threat to the common trends assumption underlying our difference-in-difference model could be if pre-treatment trends in body weight in MML states were different from non-MML states. In Table V, we present specifications that add 3 years of policy leads on the right-hand side of the estimating equation. Reassuringly, we find no evidence that body weight was trending differently in states that implemented MMLs versus those that did not in the years leading up to effective dates. Moreover, neither the magnitude nor significance of the contemporaneous or lagged MML effects were affected by the inclusion of policy leads, either in models without state trends (column 1) or with state-specific quadratic time trends (column 2).

5.2. Synthetic control analysis

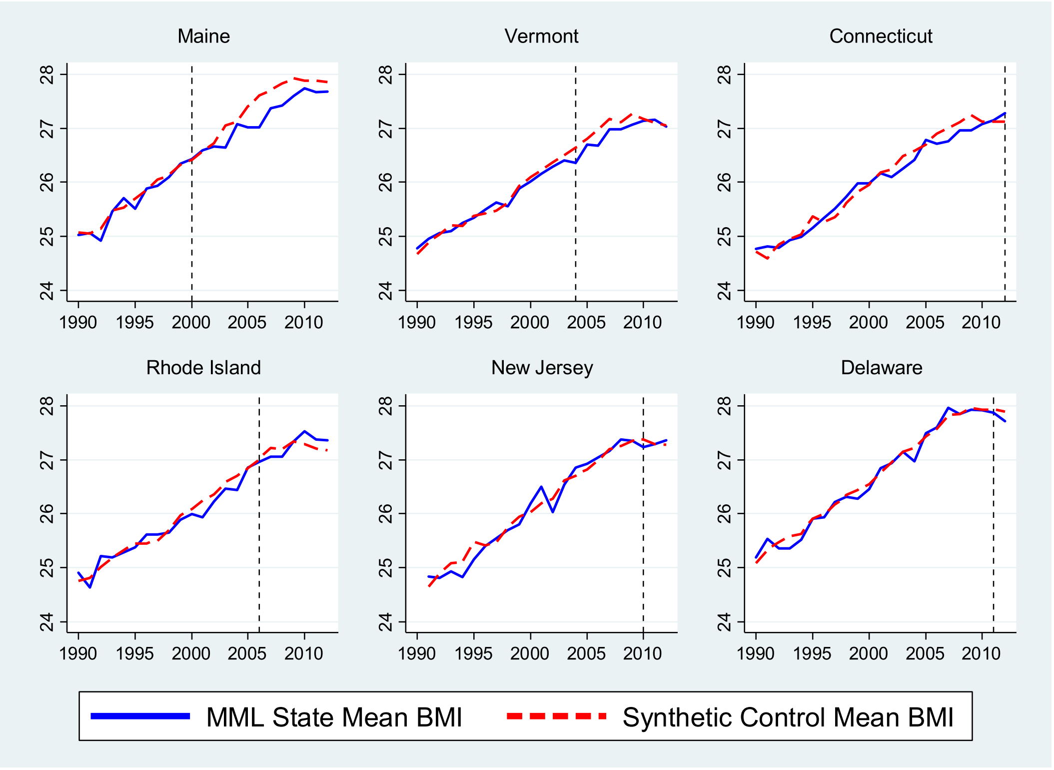
To examine the sensitivity of our estimates to the choice of counterfactuals, we generate a synthetic state for each state that implemented an MML based on pre-treatment levels and trends in body weight, race, age, average wage, at least attended college, unemployment, beer taxes, marijuana decriminalization, and cigarette taxes. As noted earlier, the counterfactual synthetic control for each treated state is generated as a linear combination of donor states that do not enforce an MML within the period covered by our sample. The weight that each donor state contributes to the counterfactual synthetic control state is chosen to minimize pre-treatment state aggregate differences across covariates in each treatment and synthetic state (Abadie *et al*. 2010). For example, the synthetic counterfactual for California is composed of 32.6% in Minnesota, 16.4% in New York, 43.1% in Utah, and 8.0% in Wisconsin. To take another example, the synthetic counterfactual for Montana is composed of 16.1% in Florida, 12.5% in Idaho, 12.6% in Oklahoma, 19.6% in South Dakota, and 39.2% in Utah. In Supporting Information Table AI, we show balancing tests of the observables in aggregated treatment and synthetic control states. The findings suggest that the synthetic control method is effective in minimizing pre-treatment differences in these characteristics.

Figures 3–5 show the estimated effects of each state's MML policy. Examining each state individually allows one to fully explore heterogeneity in MML policies across states. We find evidence that the enforcement of an MML is associated with a reduction in body weight in California, Oregon, Colorado, and Montana. One

Table V. Difference-in-difference estimates of the relationship between MMLs, BMI, and obesity: adding leads and lags

	Panel I: BMI		Panel II: obesity	
3 years before	−0.021 (0.036)	−0.01 (0.029)	−0.002 (0.003)	−0.001 (0.002)
2 years before	0.006 (0.036)	−0.007 (0.032)	−0.002 (0.002)	−0.002 (0.002)
1 year before	−0.075* (0.043)	−0.060 (0.041)	−0.004 (0.003)	−0.003 (0.003)
Year of law change	−0.103** (0.040)	−0.105** (0.049)	−0.005 (0.003)	−0.005 (0.004)
1 year after	−0.086** (0.043)	−0.095 (0.065)	−0.006* (0.003)	−0.007 (0.005)
2 years after	−0.178*** (0.051)	−0.195*** (0.070)	−0.011*** (0.004)	−0.011** (0.005)
3 years after	−0.148** (0.057)	−0.174** (0.084)	−0.011** (0.004)	−0.012** (0.006)
4 years after	−0.030 (0.056)	−0.065 (0.082)	−0.004 (0.004)	−0.005 (0.006)
5 years after	−0.177** (0.072)	−0.165* (0.092)	−0.011** (0.005)	−0.009 (0.006)
State time trends	No	Yes	No	Yes
Mean (MML = 0)	27.00	27.00	0.240	0.240
N	5,428,399	5,428,399	5,428,399	5,428,399

Note: Each column represents a result from separate unweighted regressions that include state and year fixed effects. Demographic and economic controls include gender, race (White, Black, and Hispanic), education, marital status, average wage by state and year (CPS), and state-level unemployment rate (BLS LAUS). State-level policy controls include marijuana decriminalization laws, zero tolerance laws, and state level alcohol and cigarette taxes. Food prices are collected from ACCRA and include prices for potatoes, bananas, lettuce, sweet peas, tomatoes, peaches, frozen corn, hamburger, pizza, and fried chicken. Standard errors are below each coefficient estimate in parentheses and are clustered by state. State time trends consist of interacting a linear time and a squared time variable with state fixed effects to generate a state-specific quadratic time trend.
*Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.



Notes: Plots come from synthetic control analysis for each state, where the synthetic control state is a linear combination of states that do not have MMLs in the sample. The black vertical line denotes the year a given state's MML takes effect.

Figure 3. Trends in BMI in treatment and synthetic control states

explanation for this finding is that these state MMLs were accompanied by relatively few supply-side restrictions (e.g., on dispensaries), which likely led to large spillovers into the recreational market. However, given this explanation, it is perhaps surprising to see MML-associated BMI declines in states such as Hawaii, Vermont, and Maine, which possess relatively more restrictions on dispensaries. However, a closer examination of laws in these states suggests that the allowance of home cultivation, and the difficulty in regulating individual growers (Anderson and Rees 2014), may be an important driver of MML-induced declines in body weight in these states. Subtle differences in MML policy, such as these, may be important. These observations further motivate our discussion of heterogeneity in MMLs in the succeeding discussions.

Next, we pool individuals from each treatment state and its synthetic control state to generate our sample for the synthetic control analysis.[16] In Figures 6 and 7, we present event studies using the treatment states and synthetic control states. Our synthetic figures show that both means and pre-treatment trends in body weight in the treatment and control states are nearly identical, an improvement on the event study shown in Figures 1 and 2. Following the effective date of an MML, body weight rises less rapidly in treatment as compared with synthetic states.

[16]Note that because the choice of synthetic control state for each treatment states uses observations from each potential donor state to assign weights, the sample for the synthetic control analysis is the same as for the main difference-in-difference analysis (see Brown *et al*. 2014 for a discussion of synthetic control analysis using individual-level data).

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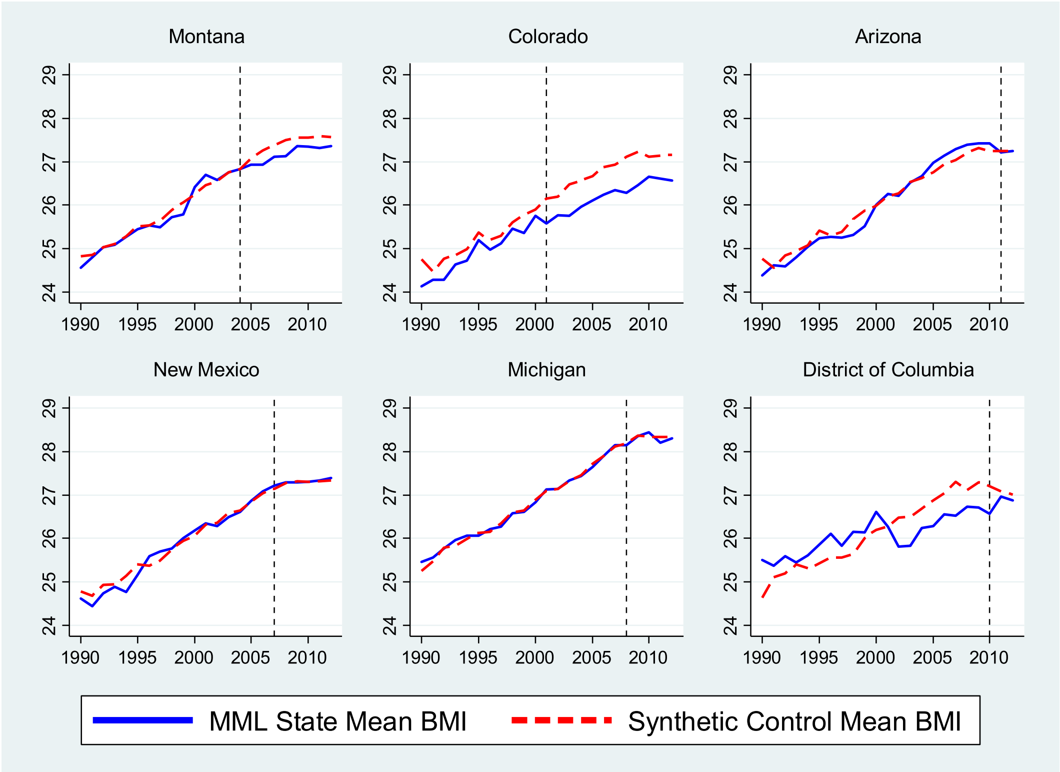

Notes: Plots come from synthetic control analysis for each state, where the synthetic control state is a linear combination of states that do not have MMLs in the sample. The black vertical line denotes the year a given state's MML takes effect.

Figure 4. Central MML states versus synthetic controls for BMI

In Table VI, we show difference-in-difference estimates using the treatment states and their synthetic weighted counterfactual states. We report bootstrapped standard errors in parentheses, where regressions are clustered at the state level. The results are quantitatively and qualitatively similar to our previous difference-in-differences analysis. Compared with our findings in Table III, column 4, the synthetic results indicate somewhat larger negative effects of MMLs on body weight, although we cannot reject the hypothesis that the estimates are statistically equivalent across the two estimation strategies. In summary, our findings provide consistent evidence that MMLs are associated with a reduction in BMI and in the probability of obesity.

5.3. Age-specific estimates

Panel I of Table VII presents the effects of MMLs on BMI by age cohort. In general, we find evidence across the age distribution that MMLs are associated with a reduction in body weight. For 18- to 24-year-olds (column 2), we find that the enactment of MMLs is associated with a 2.3% (0.577/24.59) decline in BMI 5 or more years after enactment. While statistically indistinguishable from zero, we also find a negative relationship for 25- to 49-year-olds (columns 3 and 4). And for those ages 50 years and older (column 5), we find that MMLs reduce BMI by 0.69% (0.190/27.40).[17]

[17]We experimented with examining the effects of MMLs on body weight for those ages 50 to 59 years and 60 years and older. Estimated effects of MMLs on BMI are generally larger (and more consistently statistically distinguishable from zero) for those ages 50 to 59 years as compared with those ages 60 years and older. These results are available upon request of the authors.

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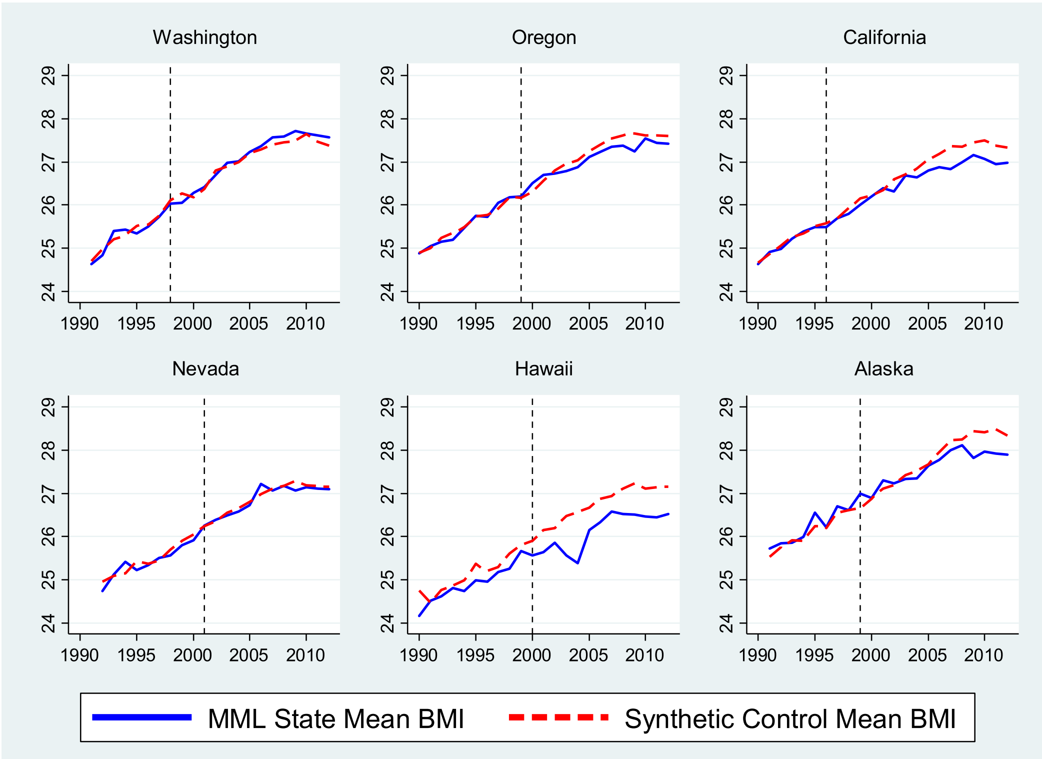

Notes: Plots come from synthetic control analysis for each state, where the synthetic control state is a linear combination of states that do not have MMLs in the sample. The black vertical line denotes the year a given state's MML takes effect.

Figure 5. Western MML states versus synthetic controls for BMI

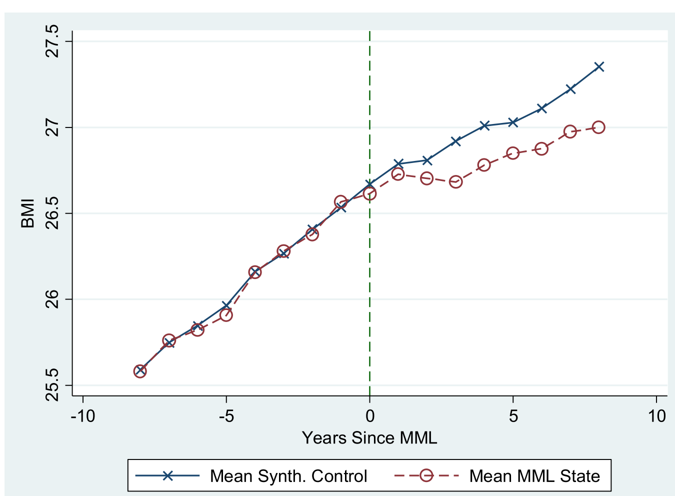

Figure 6. Event study of BMI in treatment and synthetic control states

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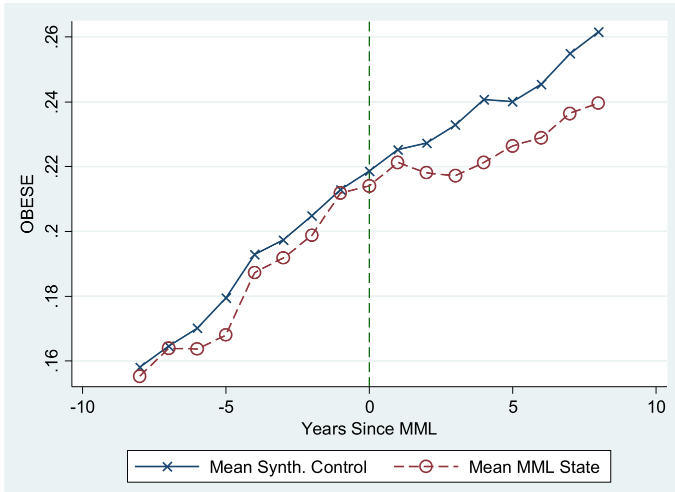

Figure 7. Event study of obesity in treatment and synthetic control states

Table VI. Synthetic control weighted difference-in-difference estimates

	(1)	(2)	(3)	(4)
Panel I: BMI				
MML	−0.088*** (0.020)	−0.075*** (0.019)	−0.066*** (0.020)	−0.098*** (0.027)
With lagged MML indicators				
Year of law change	−0.040 (0.036)	−0.029 (0.037)	−0.040 (0.036)	−0.086*** (0.033)
1 year after	−0.050 (0.035)	−0.046 (0.032)	−0.046 (0.036)	−0.086*** (0.033)
2 years after	−0.079* (0.048)	−0.076* (0.076)	−0.087* (0.047)	−0.134*** (0.040)
3 years after	−0.146** (0.059)	−0.139** (0.059)	−0.141** (0.051)	−0.184*** (0.063)
4 years after	0.019 (0.050)	0.037 (0.054)	0.051 (0.044)	−0.019 (0.047)
5 years after	−0.137** (0.026)	−0.119* (0.028)	−0.096** (0.031)	−0.101* (0.055)
Mean BMI (MML = 0)	26.214	26.214	26.214	26.214
Panel II: obesity				
MML	−0.006** (0.001)	−0.005** (0.002)	−0.004** (0.001)	−0.006** (0.002)
With lagged MML indicators				
Year of law change	0.002 (0.003)	0.002 (0.003)	0.001 (0.003)	−0.002 (0.003)
1 year after	−0.004 (0.003)	−0.004* (0.003)	−0.004 (0.003)	−0.006** (0.003)
2 years after	−0.006 (0.004)	−0.006 (0.004)	−0.006* (0.003)	−0.009*** (0.004)
3 years after	−0.013** (0.006)	−0.013*** (0.006)	−0.012** (0.005)	−0.016*** (0.005)
4 years after	−0.000 (0.004)	0.000 (0.004)	0.002 (0.003)	−0.003 (0.004)
5 years after	−0.008*** (0.002)	−0.008*** (0.002)	−0.005** (0.002)	−0.006 (0.004)
Mean obesity (MML = 0)	0.191	0.191	0.191	0.191
Demographic and economic controls	Yes	Yes	Yes	Yes
State policy controls	No	Yes	Yes	Yes
Food prices	No	No	Yes	Yes
State time trends	No	No	No	Yes
N	5,428,399	5,428,399	5,428,399	5,428,399

Note: Each column represents a result from separate unweighted regressions that include state and year fixed effects. Demographic and economic controls include gender, race (White, Black, and Hispanic), education, marital status, average wage by state and year (CPS), and state-level unemployment rate (BLS LAUS). State-level policy controls include marijuana decriminalization laws, zero-tolerance laws, and state-level alcohol and cigarette taxes. Food prices are collected from ACCRA and include prices for potatoes, bananas, lettuce, sweet peas, tomatoes, peaches, frozen corn, hamburger, pizza, and fried chicken. Bootstrapped standard errors are reported in parentheses. Sample size magnitudes are smaller than other tables because the unit of observation is now the state year. State time trends consist of interacting a linear time and a squared time variable with state fixed effects to generate a state-specific quadratic time trend.
*Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

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When we examine obesity (panel II), we find a similar pattern of results. The estimated effects appear largest for younger (ages 18 to 24 years) and older (ages 50+ years) individuals. For older individuals, as was observed for BMI, we find that 5 or more years after implementation MMLs lead to a 5.0% (0.013/0.259) reduction in the likelihood of obesity (column 5). The results in Table VII generally suggest that MMLs are effective in reducing BMI for both younger and older individuals. But could the mechanisms through which these body weight reductions differ across the age distribution? We explore this in the succeeding discussions.

5.4. Mechanisms

In Table VIII, we present results on the potential mechanisms that could explain the relationship between MMLs and body weight. Panel I shows estimates of the relationship between MMLs and the probability of experiencing poor physical health days in the last 30 days. For the full sample (column 1), we find that the enforcement of MMLs is associated with a 2.5% (0.009/0.354) reduction in the probability of experiencing at least one poor physical health day in the last 30 days. While this result is sensitive to the inclusion of state time trends, it appears concentrated on older individuals, consistent with the hypothesis of the pain-alleviating effects of MMLs. Indeed, according to registry data, medical marijuana is most commonly prescribed for chronic pain (Anderson and Rees, 2013), a condition that becomes increasingly common with age (Rustøen *et al.*, 2005; Institute of Medicine, 2011). [18] Demographic data collected from public registries support this notion and find that those most likely to obtain a prescription for medical marijuana are those aged 40 through 60 years (Doyle and Sheasley, 2012).[19] Thus, while spillovers to the recreational market are likely important for older individuals as well, there also appears to be a health-related benefit, which could indicate some pain-alleviating benefit.

Panel II of Table VIII explores the effect of MMLs on exercise at the extensive margin. There is some evidence that MMLs are associated with a reduction in exercise participation, particularly for younger individuals, consistent with the hypothesis of lethargy-inducing effects of marijuana use (Pesta *et al.*, 2013). In panel III, we present estimates at the intensive margin. While for all age groups under 50 years (columns 1 to 4), we find that MMLs are negatively related to minutes of physical exercise per week, for those ages 50 years and older (column 5), we find that MMLs are positively related to time spent performing physical exercise.[20]

When we look more closely at the intensive margin at more frequent exercise, measured as at least 90 min per week or about 15 min per day (panel IV), we find that for individuals' ages 35 years and older, the enforcement of MMLs is positively related to exercise. Specifically, for those ages 35 to 49 years, we find that MMLs are associated with a 2.7% (0.019/0.716) to 7.3% (0.052/0.716) increase in the probability of exercising at least 90 min per week (about 15 min per day). These findings provide some suggestive evidence that marijuana use—particularly for pain-alleviating purposes—may improve physical mobility for older individuals with some previous mobility.

While improved physical wellness and increased exercise (among those with some mobility) might explain part of the reduction in body weight for older individuals in our sample, what explains the reduction

[18]Colorado had 128,698 patients of whom 94% reported chronic pain in 2011. In Arizona, 86% of the medical marijuana patients suffered from chronic pain.

[19]For example, most users in Nevada are between 55 and 60 years old, while the average age for patients in Colorado and Montana is 41 and 46 years, respectively.

[20]Estimation of a two-part model via probit (extensive margin) and GLM (intensive margin) to obtain an overall exercise estimate produced a similar pattern of results as shown in panels II and III.

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Table VII. Age-specific estimates of the effect of MMLs on body weight

	All ages	18–24 years	25–34 years	35–49 years	50+ years
Panel I: BMI					
3 years before	−0.010 (0.029)	−0.213* (0.115)	−0.045 (0.069)	−0.026 (0.058)	−0.002 (0.048)
2 years before	−0.007 (0.032)	−0.121 (0.105)	0.023 (0.107)	0.040 (0.064)	−0.046 (0.040)
1 year before	−0.060 (0.041)	0.066 (0.103)	−0.100 (0.070)	−0.021 (0.055)	−0.078 (0.060)
Year of law change	−0.105** (0.049)	−0.217 (0.179)	0.063 (0.127)	−0.071 (0.077)	−0.138** (0.061)
1 year after	−0.095 (0.065)	−0.150 (0.161)	−0.106 (0.128)	0.000 (0.102)	−0.088 (0.069)
2 years after	−0.195*** (0.070)	−0.425** (0.178)	−0.088 (0.154)	−0.117 (0.103)	−0.179** (0.078)
3 years after	−0.174** (0.084)	−0.275* (0.164)	−0.098 (0.168)	−0.060 (0.113)	−0.190** (0.088)
4 years after	−0.065 (0.082)	−0.226 (0.162)	0.085 (0.164)	0.044 (0.125)	−0.090 (0.093)
5 years after	−0.165* (0.092)	−0.577** (0.222)	−0.019 (0.181)	−0.029 (0.144)	−0.190* (0.096)
Mean BMI (MML = 0)	27.00	24.59	26.22	27.21	27.40
State time trends	Yes	Yes	Yes	Yes	Yes
N	5,428,399	316,544	722,704	1,445,107	2,915,976
Panel II: obesity					
3 years before	−0.001 (0.002)	−0.010 (0.007)	−0.003 (0.005)	0.001 (0.004)	0.000 (0.004)
2 years before	−0.002 (0.002)	−0.004 (0.009)	−0.001 (0.009)	0.000 (0.004)	−0.005 (0.003)
1 year before	−0.003 (0.003)	0.007 (0.008)	−0.012** (0.005)	0.003 (0.003)	−0.005 (0.005)
Year of law change	−0.005 (0.004)	−0.013 (0.013)	0.006 (0.009)	−0.003 (0.005)	−0.007 (0.006)
1 year after	−0.007 (0.005)	−0.014 (0.011)	−0.002 (0.009)	−0.001 (0.007)	−0.007 (0.006)
2 years after	−0.011** (0.005)	−0.036*** (0.013)	−0.003 (0.011)	−0.001 (0.007)	−0.012* (0.006)
3 years after	−0.012** (0.006)	−0.013 (0.012)	−0.008 (0.011)	−0.006 (0.009)	−0.013* (0.007)
4 years after	−0.005 (0.006)	−0.016 (0.011)	−0.002 (0.012)	0.005 (0.009)	−0.006 (0.008)
5 years after	−0.009 (0.006)	−0.041*** (0.014)	0.004 (0.013)	0.005 (0.010)	−0.013 (0.008)
Mean obese (MML = 0)	0.240	0.129	0.201	0.250	0.259
State quadratic time trends	Yes	Yes	Yes	Yes	Yes
N	5,428,399	316,544	722,704	1,445,107	2,915,976

Note: Each column represents a result from separate unweighted regressions that include state and year fixed effects. Demographic and economic controls include gender, race (White, Black, and Hispanic), education, marital status, average wage by state and year (CPS), and state-level unemployment rate (BLS LAUS). State-level policy controls include marijuana decriminalization laws, zero-tolerance laws, and state-level alcohol and cigarette taxes. Food prices are collected from ACCRA and include prices for potatoes, bananas, lettuce, sweet peas, tomatoes, peaches, frozen corn, hamburger, pizza, and fried chicken. Standard errors are below each coefficient estimate in parentheses and are clustered by state. State time trends consist of interacting a linear time and a squared time variable with state fixed effects to generate a state-specific quadratic time trend.
*Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

in body weight for the younger cohorts? One answer could be the alcohol effects of MMLs. For those ages 18 to 24 years, our findings show that the enactment of MMLs is associated with a 3.1% (0.017/0.551) reduction in the probability of alcohol consumption (panel V, column 2) and a 4.8% (0.013/0.269) reduction in the probability of binge drinking (panel VI, column 3). Therefore, MMLs may induce younger individuals to substitute away from highly caloric alcoholic beverages toward a lower-calorie marijuana ‘high’, resulting in lower body weight and likelihood of obesity. There is also some evidence of substitution effects among older individuals, suggesting a recreational component to MML-induced marijuana use for these individuals.

In panel VII, we find some evidence that MMLs are associated with reductions in food consumption. Taken at face value, these results do not seem consistent with the hypothesis that marijuana induces greater appetite. However, it is important to note that our measures for consumption primarily capture the consumption of relatively healthy foods. It is possible that MMLs reduce the consumption of healthy food as individuals substitute toward less healthy, higher-calorie alternatives (Kirkham, 2009; Foltin *et al*., 1988).

Finally, in panel VIII, we find evidence a negative relationship between MMLs and the probability that a respondent has reported any poor mental health days in the last 30 days, consistent with Anderson, Rees, and Sabia (2014), although this finding is somewhat sensitive to the inclusion of state time trends. This could suggest that improved mental health improves efficiency of production of

Table VIII. Examining mechanisms through which MMLs may affect body weight

	All ages		18–24 years		25–34 years		35–49 years		50+ years	
Panel I: any poor physical health days										
Poor physical health days	−0.009* (0.005)	−0.010 (0.006)	−0.011* (0.007)	−0.007 (0.013)	−0.003 (0.006)	−0.012 (0.011)	−0.008 (0.006)	−0.018** (0.009)	−0.009* (0.005)	−0.005 (0.004)
Mean (MML = 0)	0.354		0.354		0.328		0.343		0.368	
N	5,176,842		287,599		669,034		1,377,555		2,802,428	
Panel II: any minimal physical exercise										
Any minimal exercise	0.008 (0.010)	−0.025* (0.013)	0.004 (0.013)	−0.071 (0.049)	−0.002 (0.019)	−0.022 (0.016)	0.005 (0.011)	−0.035* (0.009)	0.014 (0.009)	−0.018 (0.012)
Mean (MML = 0)	0.388		0.583		0.529		0.476		0.294	
N	1,650,516		83,103		203,563		448,655		902,953	
Panel III: minutes of exercise (*conditioned on any minimal exercise*)										
Minutes of exercise	−2.029 (4.831)	−9.452 (8.840)	0.948 (15.212)	−27.596 (33.289)	−11.026 (8.335)	−25.316 (20.747)	−6.748 (8.440)	−17.006 (8.499)	2.228 (4.198)	2.281 (15.062)
Mean (MML = 0)	223.55		260.91		209.98		211.78		231.83	
N	665,592		49,504		109,943		220,050		281,655	
Panel IV: at least 90 min per week (*conditional on any minimal exercise*)										
Exercise ≥ 90 min per week	0.011** (0.005)	0.025*** (0.008)	0.005 (0.016)	0.019 (0.025)	−0.006 (0.016)	0.019 (0.028)	0.019* (0.028)	0.052*** (0.015)	0.011* (0.005)	0.008 (0.014)
Mean (MML = 0)	0.725		0.758		0.724		0.716		0.726	
N	665,592		49,504		109,943		220,050		281,655	
State time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel V: consumed any alcohol in the last 30 days										
Drink anything	−0.009 (0.006)	−0.006 (0.005)	−0.021** (0.010)	−0.017** (0.008)	−0.016* (0.009)	−0.008 (0.008)	−0.002 (0.007)	−0.009 (0.005)	−0.009 (0.005)	−0.003 (0.005)
Mean (MML = 0)	0.486		0.551		0.583		0.562		0.418	
N	5,181,180		286,441		667,771		1,359,434		2,828,214	

(*Continues*)

Table VIII. (Continued)

	All ages		18–24 years		25–34 years		35–49 years		50+ years	
Panel VI: any binge drinking in the last 30 days										
Binge drinking	−0.009*** (0.003)	−0.005** (0.002)	−0.021** (0.007)	−0.013* (0.007)	−0.010 (0.006)	−0.006 (0.006)	−0.010** (0.004)	−0.007* (0.004)	−0.005** (0.002)	−0.003* (0.002)
Mean (MML = 0)	0.118		0.269		0.213		0.155		0.061	
N	5,127,666		238,118		661,835		1,3466269		2,798,323	
Panel VII: food consumption										
	All ages		18–24 years		25–34 years		35–49 years		50+ years	
Appetite	0.976 (1.133)	−2.527 (1.611)	1.647 (1.441)	−3.49** (1.505)	2.035 (1.619)	−2.764 (2.434)	1.221 (1.227)	−3.138* (1.578)	0.955 (1.108)	−2.045 (1.634)
Mean (MML = 0)	117.86		107.69		110.27		113.36		124.15	
N	2,262,538		140,954		332,665		657,606		1,117,259	
Panel VIII: had any poor mental health days in the last 30 days										
Mental health	−0.013* (0.008)	−0.001 (0.010)	−0.038** (0.014)	−0.016 (0.019)	−0.021* (0.011)	−0.000 (0.018)	−0.013 (0.010)	−0.001 (0.012)	−0.004 (0.005)	−0.002 (0.006)
Mean (MML = 0)	0.315		0.458		0.399		0.375		0.251	
N	5,191,627		287,738		667,574		1,374,420		2,821,617	
State time trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Note: Each column represents a result from separate unweighted regressions that include state and year fixed effects. Demographic and economic controls include gender, race (White, Black, and Hispanic), education, marital status, average wage by state and year (CPS), and state-level unemployment rate (BLS LAUS). State-level policy controls include marijuana decriminalization laws, zero-tolerance laws, and state-level alcohol and cigarette taxes. Food prices are collected from ACCRA and include prices for potatoes, bananas, lettuce, sweet peas, tomatoes, peaches, frozen corn, hamburger, pizza, and fried chicken. Standard errors are below each coefficient estimate in parentheses and are clustered by state. State time trends consist of interacting a linear time and a squared time variable with state fixed effects to generate a state-specific quadratic time trend.
*Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

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Table IX. Exploring heterogeneity in MMLs

Panel I: BMI	(1)	(2)	(3)	(4)	(5)
Any MML	−0.084** (0.034)				
MML for pain		−0.088*** (0.030)			−0.063** (0.030)
MML with collective cultivation allowed			−0.110** (0.054)		−0.072 (0.054)
MML that allows for dispensaries				−0.081 (0.051)	−0.011 (0.026)
Mean BMI (MML = 0)	27.00	27.00	27.00	27.00	27.00
Panel II: obese					
Any MML	−0.005* (0.003)				
MML for pain		−0.004** (0.002)			−0.003 (0.002)
MML with collective cultivation allowed			−0.005 (0.004)		−0.003 (0.003)
MML that allows for dispensaries				−0.005 (0.004)	−0.001 (0.002)
Mean obese (MML = 0)	0.240	0.240	0.240	0.240	0.240
State time trends	Yes	Yes	Yes	Yes	Yes
N	5,428,399	5,428,399	5,428,399	5,428,399	5,428,399

Note: Each column represents a result from separate unweighted regressions that include state and year fixed effects. Column (1) presents the coefficient estimates of the standard measure of MML used in this paper, following the interpretation of MML effective dates by Anderson *et al*. Column (2) is a measure of MMLs (effective dates from Anderson and Rees (2013)) that allow for pain according to Pacula *et al*. (2013). Column (3) estimates the effect of MMLs that allow for collective cultivation according to Anderson and Rees (2013). Column (4) estimates the effect of MMLs for states that have at least one operating dispensary following coding by Pacula *et al*. (2015). Demographic and economic controls include gender, race (White, Black, and Hispanic), education, marital status, average wage by state and year (CPS), and state-level annual unemployment rate (BLS LAUS). State-level policy controls include marijuana decriminalization laws, zero-tolerance laws, and state-level alcohol and cigarette taxes. Food prices are collected from ACCRA and include prices for potatoes, bananas, lettuce, sweet peas, tomatoes, peaches, frozen corn, hamburger, pizza, and fried chicken. Standard errors are below each coefficient estimate in parentheses and are clustered by state. State time trends consist of interacting a linear time and a squared time variable with state fixed effects to generate a state-specific quadratic time trend.
*Significant at 10% level; **Significant at 5% level; ***Significant at 1% level.

physical health. However, it is also possible that improved mental health could be a consequence of lower body weight. [21]

5.5. Heterogeneity in MMLs

The results from our synthetic cohort analysis suggest that there may be heterogeneous effects of MMLs on body weight. We explore this issue further in Table IX. Column (1) shows estimates of the effect of the average MML on body weight. In the remaining columns, we explore the effects of state MMLs that allow (i) medical marijuana to be prescribed for pain (column 2), (ii) collective cultivation of medical marijuana (column 3), and (iii) medical marijuana dispensaries (column 4). Column (5) includes controls for each of these types of MMLs in the same regression.

We find that MMLs that are pain-inclusive (column 2) and allow for collective cultivation of marijuana (column 3), which generates the largest negative body weight effects (see Supporting Information Table AII for examples of allowable conditions). These findings are consistent with the

[21]In results available upon request, we explore the relationship between MMLs and the probability of visiting a primary care physician in the last year. We find little evidence that MMLs affect contact with primary healthcare providers. However, there are a number of important limitations with this measure. Patients seeking a prescription for marijuana might choose to visit a medical marijuana evaluation clinic rather than their primary care physician, where doctors regularly write medical marijuana recommendations (Reinarman *et al*., 2011). Additionally, if patients are seeking a medical marijuana recommendation for a particular injury, illness, or condition, they may not necessarily visit their primary care physician. The BRFSS does not include data on other contact with healthcare providers.

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hypotheses that (i) there may be some physical mobility benefits of MMLs, and (ii) allowing the home cultivation and distribution of marijuana to other patients (on a non-profit basis) may have spillover effects into recreational markets. The estimated effect of MMLs that permit marijuana dispensaries also suggest negative effects on body weight (column 4), although these estimates are statistically indistinguishable from zero.[22]

6. CONCLUSIONS

This paper is the first to examine the effect of MMLs on body weight, physical well-being, and exercise. Difference-in-differences estimates suggest that the enactment of MMLs lead to a 0.3% to 0.6% decrease in BMI scores and a 2.1% to 6.0% decline in the likelihood that respondents report being obese. We also find evidence of heterogeneous mechanisms for body weight reduction across the age distribution. For older individuals, we find that MMLs are associated with an increase in physical wellness and frequent exercise consistent with the hypothesis of some medicinal use of marijuana. However, the magnitude of the overall body weight effect suggests some spillover effects into the informal 'non-medical' marijuana market as well. For younger individuals, MML-induced reductions in alcohol consumption appear to be relatively more important, consistent with the hypothesis that MMLs lead to substitution toward a less caloric recreational high.

The estimated body weight effects we obtain should be interpreted as 'intent to treat' (ITT) estimates. Because the BRFSS does not include information on marijuana consumption, our approach does not immediately yield estimates of the effect of MMLs on individuals who are induced to use marijuana because of MMLs. Obtaining the implied average effect of treatment on the treated (ATET) estimates from our ITT estimates requires knowledge of the effect of MMLs on marijuana use (Angrist & Pischke, 2009, p. 164). Wen *et al*. (2014) estimate that MMLs increase marijuana consumption by 16% on the extensive margin and 17% on the intensive margin (33% total increase) of use among individuals over age of 21 years. Using their estimate, we obtain implied bounds for ATETs indicating a 0.9% to 1.8% decline in BMI and a 6.3% to 13.0% decrease in the likelihood of obesity. These estimates are actually 45% to 61% smaller in magnitude than those obtained by Le Strat and Le Foll (2011), who document obesity prevalence to be 16% to 23% lower among marijuana users.

Using estimates from Cawley and Meyerhoefer (2012), we estimate a back-of-the-envelope per-person reduction in MML-induced obesity related medical costs of \$58 to \$115 per year.[23] These, too, could be rescaled by a factor of 3 to 5 (from first stage of Wen *et al*. (2014) and Anderson and Rees (2013), respectively) if we believe compliers drive these effects. However, note that there are limitations to this partial equilibrium calculation. MML-induced reductions in obesity-related expenses may be offset by other types of medical expense because of general equilibrium effects, some directly related to marijuana use. Future work (with richer data) is needed in order to explore the impact of MMLs on other health outcomes and to conduct a more complete welfare analysis.

[22]The coding of dispensary states is the subject of debate because of the heterogeneity in the legality and operation of dispensaries in different states (Anderson and Rees, 2014). For our analysis, we follow the coding outlined by Pacula *et al*. (2015), in which an MML is considered dispensary inclusive if the legislation contains legal language allowing for the operation of dispensaries and have at least one dispensary in operation. There are important limitations to coding MMLs in this fashion. Even though a state may statutorily allow for dispensaries, state agencies may strictly regulate them, allowing very few to operate; for instance, there may be statutory limitations on the number of dispensaries that are allowed to operate, such as in Maine. Moreover, the enforcement of federal anti-marijuana laws by federal agents may affect the number of operating dispensaries (Mikos, 2011). While using counts of dispensaries may provide better information on the availability of marijuana in a state, they also likely capture demand-side factors, which could exacerbate endogeneity bias if unmeasured factors that affect the demand for marijuana also affect health.

[23]This is obtained by taking the Cawley and Meyerhoefer (2012) estimate, \$2,741, and multiplying by estimates from Table 4, row 1, column 4 (which gives the lower bound) and column 1 (which gives the upper bound).

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CONFLICT OF INTEREST

None of the authors have a financial interest in the policy being discussed in this manuscript. This study uses secondary data, and there are no ethical conflicts with this study.

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